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THE PSYCHOLOGICAL REVIEW.

THE WORLD AS MECHANISM.

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The analyses of the psychologist and of the metaphysician reveal to us that the real world in space and time is an orderly system of things given in terms of touch and movement sensations. This is the world of matter in motion which the science of mechanics attempts to describe to us. It is quite possible to treat of it intelligently without being either psychologist or metaphysician, for one may confine oneself to certain aspects of it without attempting to discuss certain others. When a physicist loosely describes matter as 'everything that one can touch,' and then busies himself with the changes that take place in the world of matter, ignoring all epistemological problems, he confines himself to a definite field of investigation, and the results he obtains within that field need not be at all vitiated by the fact that he neither raises nor suggests certain other questions with which other men busy themselves. Without leaving the plane of the common understanding, he may ask himself whether he is to look upon the material world as through and through a mechanism, or whether he must abandon this conception as being unsatisfactory. He has a right to expect that the arguments *pro* and *con* will be such as to appeal to men of intelligence who are not devoted adherents of this or that metaphysical theory.

Notwithstanding the fact that a series of eminent names may be cited as favoring the opposite doctrine, the statement does not appear unwarranted that the man of science, as such, is

coming to incline more and more to the view that the changes which take place in the world of matter form an unbroken series and are all explicable according to mechanical laws. It ought to be frankly admitted by everyone that the material world is not *known* to be such a system. We may, indeed, conceive it to have swept through an unbroken series of changes, from the cosmic mist in which our ignorance looks for its beginnings, to the organized whole in which vegetable and animal bodies play their part; but, even as we call before us the vision, we realize that it is revealed only to the eye of faith, and is but dimly discerned through the obscurity which enshrouds it. Even if we leave out of view the difficulties connected with the structure of the atom and the nature of the ether, we are forced to admit that scarce so much as a beginning has been made in the direction of a mechanical explanation of the combination of atoms into molecules and the origin of the kinds of matter of which, as the chemist informs us, our world is made up. Given, too, the chemical elements and the laws of their combination as empirically known to the chemist, we still search in vain for an explanation of the phenomena of living organisms, and fail to account for their appearance upon this planet. Chemistry, physics, biology—these are as yet relatively independent realms, and it remains for a perhaps far distant future to give them all a solid basis in mechanics, and thus to unite our present fragmentary glimpses into the nature of things into a reasonable and comprehensive whole. We have a collection of sciences whose relations to each other are not clearly seen. We have not yet a science which can string on a single thread the beads that we have with such labor collected together.

But it is one thing to admit our present ignorance, and it is quite another to maintain that it is, in the nature of things, ultimate and irremovable. The steady growth of science encourages those who are imbued with the scientific spirit to hope that, in our knowledge of nature, discontinuity will gradually give place to continuity, and that there will become more and more clear before our eyes an orderly mechanical system, the successive stages in the evolution of which will not have to be accepted as inexplicable fact, but will be seen to be the appro-

priate steps in a series of changes, the inevitable succession of which we may infer with confidence, and which we are unable to comprehend only where we are still hampered by our ignorance. That this faith in the mechanism of nature is justified cannot be proved by the philosopher in his closet. It can be proved only by the actual extension of our knowledge of nature, and until this has taken place, the doctrine can be no more than a working hypothesis. It is, however, sometimes urged that it should not be held even as a working hypothesis, and various considerations are brought forward to prove that the doctrine is inherently absurd. Upon certain of these I shall dwell briefly in what follows.

I.

If one may judge by the comments and reviews in a considerable number of the more popular journals, many persons have followed with pleasure the recent attempt made by Dr. James Ward to prove that the science of mechanics is no true science at all, and that its fundamental concepts, when carefully examined, turn out to be self-contradictory and absurd.[1] I do not propose here to criticise Dr. Ward's reasonings in detail. I wish merely to point out that they seem to arise, in part, at least, out of a misconception of the nature of the science of mechanics and of the foundations upon which it rests. That even in dealing with the most common things about us, the movement of a rolling ball, the raising of a weight with the aid of a lever, the rotation of a wheel upon its axle, all our measurements are only approximately correct, and fall short of the exactitude demanded by scientific theory, no one need trouble himself to deny. That certain scientific concepts, which are undoubtedly useful within a given sphere, may, when carried over to or beyond the confines of our knowledge, and employed in those somewhat fanciful constructions by the aid of which the man of science hopes to win at least a glimpse of truths which he must frankly admit lie beyond his horizon—that such concepts may sometimes be found inadequate to such uses, need not be found surprising. We all listen with more curiosity than

[1] 'Naturalism and Agnosticism,' Part I.

conviction to what certain speculative minds have to tell us of the ultimate constitution of matter; and those who speak, if they be men of sense, do not pretend to be clothed with the spirit of prophecy. But all this has nothing to do with the question whether a science is a true science, and rests upon a reasonably secure foundation.

It should never be forgotten that the science of mechanics, like other sciences, has its foundation in our common experience; that it is merely the systematization, refinement and extension of our ordinary knowledge of things and their motions. The savage, who uses a stick to pry a stone out of its setting, the boy who throws a bit of coal at a cat, even these have made a beginning in the knowledge of a mechanical system of things. That no little advance has been made from such a beginning is patent to anyone familiar with contemporary science. The notion of mechanism is a perfectly familiar one, and to it we constantly turn for an explanation of changes which we perceive to be taking place in the world about us. Whatever may become of the doctrine of atoms and molecules, it remains true that we can calculate with some degree of accuracy the position of the moon with reference to the earth on a particular day and hour, and we can trace with some accuracy the path of a projectile. Whether we may not justly expect to find in the notion of mechanism the explanation of all the changes that take place in the material world, is a question that it is by no means absurd to raise, even when one is not at all in a position to prove that all changes in matter are mechanical. One may raise the question, and may be inclined to give it an affirmative answer, although one be in doubt whether any proposed theory of the intimate constitution of matter be the correct one. Very early in the history of speculative thought it occurred to men's minds that those things which, by reason of their minuteness, are concealed from our view, might be reasoned about by analogy with those things which are more open to inspection. With the principle itself we can have no quarrel. We act upon such principles in every department of human thought. It is, of course, important that we should not reason loosely, and should not too hastily arrive at conclusions. And if any as-

sumptions which we have been impelled to make should turn out upon closer inspection to entail consequences which we cannot accept, we should know how to repudiate those assumptions without tossing overboard with them that whole body of observed facts and well-grounded generalizations which have established their right to be regarded as a science, if only an imperfect one. There is such a body of facts and generalizations that constitutes the science of mechanics. To laugh at this science because it has its limitations is absurd, and it is a complete misconception to suppose that a science must be completed before it can have a foundation. In the present instance, it is the apex of the pyramid that is hid in clouds, not its foundation, for this lies in plain view, and no man can afford to despise it.

To the metaphor of the pyramid it may be objected that, when one is dealing with a science, a view of the apex may very materially modify one's notions of the extent and outline of the base. This is quite true. A science does not grow by mere accretion. The discovery of a new truth may necessitate a modification of generalizations which seemed amply justified when our information was more limited. The Newtonian laws of motion, for example, have justly been regarded as a triumph of modern science, and as lying at the very foundation of the science of mechanics. Nevertheless, we are now told[1] that they will probably have to be modified if we wish to describe accurately the material universe as it seems to be revealing itself to the inquisitive eye of the modern investigator. Does this mean that the observations and reflections which resulted in those laws are to be regarded as valueless, and the laws themselves to be repudiated as utterly erroneous? By no means.

It means only that a truth imperfectly apprehended has come to be more perfectly apprehended. Such a growth in human knowledge is not revolution; it is entirely in harmony with the natural and normal development of the sciences. What are commonly called their fundamental principles or concepts are not fundamental in the sense that they must be definitely

[1] Pearson, 'The Grammar of Science,' 2d ed., Chap. VIII.

established and placed beyond the possibility of being called in question, before the science can be built up at all. Such principles or concepts are the ideal of a completed science, if such a term may be used. They are not to be found in a science in the making. They are the goal toward which a science strives, and not the ground which it actually feels beneath its feet. Hence one may freely admit that men of science are not at one touching the final definition of matter, and are not agreed upon the proper formulation of the laws of motion, without on that account being compelled to deny that there is such a science as mechanics, and that in it we find a satisfactory explanation of a vast number of the changes which we observe to be taking place in the world. And one may make these admissions without being compelled to abandon the hope that, with the extension of human knowledge, a vast number of other changes, which cannot now be seen to find their explanation as these do, may be found to fall in the same general class, and may become luminous with a significance now denied to them. It is sheer dogmatism to insist that the material world cannot be a perfect mechanism, merely on the ground that, in the present state of our knowledge, it cannot be proved to be such. What we should ask ourselves is this: What, on the whole, is it reasonable for us to believe, and with what degree of assurance should we believe it? He who is accustomed to weigh evidence, and who realizes the limitations of our actual knowledge, will take his position on such a subject tentatively, and will hold himself in readiness to abandon it when good reason is adduced for his doing so.

The detailed discussion of Dr. Ward's strictures upon the science of mechanics may safely be left to the intelligent student of natural science. A number of them are, I think, clearly open to objection upon purely scientific grounds. But there is one general consideration touching the attitude of Dr. Ward and of a sufficiently large number of other persons toward the mechanical view of the system of nature, that is of no little significance, and that has not, I think, received due emphasis. It is this: The energetic rejection of the doctrine that the material world may be regarded as a perfect mechanism appears

to arise (if one may judge by what is written upon the subject) out of the conviction that such a view of the world militates against certain beliefs to which men cling with a good deal of energy and which they relinquish with reluctance. We do not find that attacks upon the conception of mechanism are wholly destructive in their aim. Those who cannot find in mechanics an explanation of the changes which take place in the material world, are inclined to find such an explanation in the action and interaction of minds. They do not merely abandon a proposed view of nature because they find it unsatisfactory, and content themselves with holding no view at all. They abandon one view to take up with another. It seems just to ask oneself whether, if there were the same emotional bias against the second view that appears to exist against the first, it would be found so satisfactory as many seem to find it? Are there no difficulties connected with the second view? Do we there find everything clear and comprehensible?

Let us suppose, for example, that we discover, upon reflection, that the conception of matter remains to us obscure; that we can gain no very clear notion of what is meant by mass: that we are more or less in the dark as to how the idea of causality can be connected with the changes in the material world; that the laws of motion, as at present formulated, do not seem to us to account satisfactorily for the behavior of all material particles in the presence of each other. Shall we on this account repudiate the science of mechanics, and give up all attempts at a mechanical explanation of the changes which take place in the world of matter? If so, what should we do in the case of mind? Are there no disputes as to the ultimate nature of the mind? Is there a science, or even the beginning of a science, that sets forth with any approach to clearness the relation of mind to matter, and the method by which minds act upon material particles or upon each other? Is it more evident what is meant by causal efficiency when one speaks of minds than when one speaks of masses of matter? 'Intersubjective intercourse' is a sounding phrase that calls our attention to the fact, recognized in our common experience, that, *in some sense*, minds stand in relation to each other. But in what sense? How

can a mind be related to another? Has the vague knowledge of the plain man really been replaced by something that has a right to be regarded as science? Surely the science of mechanics, unsatisfactory as it may be, has progressed far beyond our knowledge of mind, of its relations to matter, and of its relations to other minds. Here we see in a glass, darkly; each man is busied with his own speculations, and they are worth all the labor which he devotes to them. But a science we have not, unless we extend the meaning of the term so as to cover those tentative gropings for the truth which precede established knowledge. To find fault with the science of mechanics, and to take up with the vague notions which men have of minds, the activity of minds, the relation of minds to matter, and their relation to each other, is about as sensible as it would be to reject the refinements of the developed science of mechanics and take up with the crude mechanical notions possessed by the uneducated. That material things act upon one another, and that minds act and react, the plain man does not doubt. He sees nothing incomprehensible in the premises on the one side or on the other. It is the philosopher who becomes conscious of the inadequacy of his conceptions, and whose reflections sometimes tempt him to reject them altogether. But to treat one class of conceptions in the critical spirit of the philosopher, and to accept the other with the naïveté of the unreflective, is surely inadmissible. If, then, it is right to lay great emphasis on the difficulties which suggest themselves when one undertakes a critical investigation of the fundamental concepts of the science of mechanics, it must be equally just to emphasize the difficulties which arise when one endeavors to make quite clear to oneself what is meant by minds, their relation to material things, and their relation to each other. If one insists upon clearness and consistency in the former field, and is content to get along without it in the latter, it must be either that, in the latter field, the attainment of exact knowledge is looked upon as, in the nature of things, hopeless; or that the deficiencies of our knowledge are hidden from us by an emotional bias that inclines us strongly to adopt certain doctrinal statements whether they are clear to us or not.

II.

Since, therefore, it is possible for a science to be a true science before it has attained ideal completeness, it does not seem wholly unreasonable for one to assume, as a working hypothesis, at least, that the material world is a mechanism all the changes in which can be accounted for without passing beyond it to something else. Let us make such an assumption. In the orderly succession of the states which constitute the life-history of this organization we have the physical order of causes and their effects. It is, of course, clear that our knowledge of physical causes and their effects must be imperfect as our knowledge of the world-mechanism is imperfect. The boy who strikes a dog with a stick recognizes the answering yelp as a consequent, to which the movement of the stick is a corresponding antecedent. The physiologist interpolates an extremely complicated series of occurrences between the two, and regards the blow as by no means a proximate cause, while admitting it as a member in the causal nexus. Both recognize the relation of cause and effect, but to the latter the whole system has become a vastly more complicated thing than it is to the former. And the metaphysician, who may come to the conclusion that there is no assignable limit to a possible increase in the minuteness of our knowledge of the real world and its changes, may not unreasonably deem it absurd to use the expression 'proximate cause' in any but a relative sense. Still, he has the right to use it to indicate an antecedent which, in the actual state of our knowledge, seems to be nearest to a given consequent.

In the relation of cause and effect, when thus conceived, there seems to be nothing very occult or mysterious, though one must, of course, admit that we have but an imperfect knowledge of causes and their effects. The conception of causality seems, however, to be a stone of stumbling to some, and it is well worth while to devote some time to its analysis, notwithstanding the fact that it has been discussed, and well discussed, by various writers. In connection with it there appear to arise some very general misconceptions, and misconceptions which may materially modify one's view of the mechanism of nature.

Our text-books on logic warn us that the custom, which prevails in common life, of picking out that element in the total antecedent of a given occurrence which happens to be for some reason of peculiar interest or importance, and of calling it *the cause*, may easily occasion mistaken notions of cause and effect. We point out that Smith is the cause of the accident that happened to Jones, in that he handled his gun carelessly. Jones himself we do not speak of as contributing to the result. Yet it is quite clear that a man cannot be shot *in absentia*, and the bodily presence of the injured man was an indispensable part of the antecedent if the occurrence were to take place at all. The total antecedent of a given occurrence may be a thing of infinite complexity, and yet we are usually content to adduce one or two elements in it and overlook the rest. We attribute an outbreak of the plague to the visit of a ship from an oriental port, the prosperity of a country to the dominance of a political party, a happy marriage to an accidental meeting in a country house, a deadly feud to a chance word spoken in an unguarded moment. Even so we speak of a general as gaining a battle, and of a wise ruler as giving peace and prosperity to his subjects. There is no objection to our thus speaking in common life, but it is necessary to observe that, when we pass to the scientific contemplation of the order of nature and inquire regarding the explanation of given collocations of matter and motions of matter, we must view things with a more impartial eye and must not overlook the most humble contributor to the total result.

Again. In common life we are accustomed to emphasize the distinction between agent and sufferer. At times we regard ourselves as actively bringing about changes in other things, and at times we deplore the fact that external things bring about changes in us. We look upon ourselves as active when we move along the street in pursuance of a desired end, and as wholly passive with respect to the falling tile that unexpectedly interrupts our progress. This distinction we carry over to things inanimate, and the notions of activity and passivity are apt to become more or less confused with those of cause and effect.

But in the conception of nature as mechanism this distinction between active and passive wholly vanishes. The moving billiard-ball comes in contact with the ball at rest. The former comes to rest and the latter is set in motion. We are at first inclined to regard the one as active and the other as the passive recipient of its activity. But a little reflection and the most elementary knowledge of mechanical laws make clear to us that the second ball has affected the first quite as much as the first has affected the second. A series of changes has taken place in the spatial relations of certain masses of matter, and it is only through misconception that we can regard a single mass of matter as responsible for the series of changes as a whole. When we do so we are carrying over to a field in which it has lost its significance, a conception which has its legitimate application only in another field.

The same reasoning may be applied to the case of the boy striking the dog. If we will regard boy, dog and stick as merely a part of the material system of things, as collocations of matter the changes in which take place according to mechanical laws, it is impossible to look upon the boy as active and the dog as the passive recipient of his action. When we do so regard them we are employing conceptions which have a significance only in the subjective world of desires and volitions, a world with which we have nothing to do so long as we confine our attention to the material universe and its motions. To the eye with its field of view thus circumscribed, nothing is present save certain groupings of material particles which pass through a series of changes in their relative positions. The notions of activity and passivity have disappeared, but not so the notions of cause and effect. The changes through which the whole system passes are explicable according to the laws of mechanics, and each antecedent condition is the cause of the one which immediately follows it. The relation of cause and effect is a temporal one, and marks the order of the successive states in the life-history of the system; it is not a spatial one, which separates off one part of the system from another part. In other words, the boy and the stick cannot be made in some sort an antecedent, and the dog a consequent; but boy, stick and dog

are all antecedent, and are all consequent as well—the former at the one instant, and the latter at the next. The erroneous popular judgment which would make the boy the sole cause of the dog's yelp, seems to arise from a double error: the attention is fixed upon a part of the total antecedent to the exclusion of the rest; and there is present the mistaken notion that only that can be a cause which is 'active.' The popular judgment is not without its justification from a practical point of view. It is not a mere accident that men come to think and speak thus. Nevertheless, the popular judgment is shot through with misapprehension and confusion, which should, in scientific discussions, be eliminated. The notions *cause* and *activity*, *effect* and *passivity*, should be carefully divorced from one another when we concern ourselves with an exact description of the changes which take place in the material world. That the notions activity and passivity are of the utmost significance in their proper field, one may freely admit. But it is important to bear in mind that, when we are studying the successive positions of matter in motion, we have nothing to do with them at all.

It is an imperfect apprehension of the distinction between causality and activity that has misled certain writers, scientific and anti-scientific,[1] into thinking that natural science should drop altogether the notion of causality, and in place of an *explanation by a reference to causes*, substitute a *description* of the orderly series of changes that take place in the world of matter. Just so long as he confuses causality with activity, will the student of mechanics, who sees clearly that the notion of activity has no place in his science, be inclined to deny that he has to do with causes and their effects. It is because he still thinks of "an explanation by a reference to causes" as something occult and mysterious—as a procedure akin to the blind gropings behind the veil of phenomena popularly attributed to the metaphysician—that he repudiates such explanations altogether and confines himself to what he calls 'description.' But it is unwise to discard terms which for centuries have served a useful purpose, which are firmly rooted in men's minds and are fairly well

[1] *E. g.*, Mach, 'Popular Science Lectures,' English trans., pp. 253–254, and Ward, 'Naturalism and Agnosticism,' Lect. II. and XVII.

understood even by those who cannot subject them to careful criticism, and which have no satisfactory equivalents but leave a gap when they are discarded. For such terms one cannot substitute terms with other associations without giving rise to suspicion and misunderstanding. It is far better to correct popular misconceptions of the proper significance of words in common use, and point out how such words may find their appropriate application. To insist that science has nothing to do with the indication of causes and their effects, when for centuries that has been supposed by its votaries to be its chief occupation, can only occasion bewilderment. To show, on the other hand, that there has been some error as well as some truth in the popular apprehension of the natural order of causes and their effects need not have this unfortunate result.

That the relation of cause and effect has in the history of science been but imperfectly grasped is sufficiently clear. To see how dimly it has been apprehended even by men of commanding genius and thoroughly imbued with the scientific spirit, it is only necessary to read what Descartes has to say of effects as contained 'formally' or 'eminently' in their causes. Where, he asks, can an effect get its 'reality' if not from its cause? and he appears to think of this 'reality' as a measurable stuff that can be passed over from cause to effect.[1] Notwithstanding all this, Descartes had no mean conception of the material universe as a mechanical system, and no one can accuse him of more than a partial misconception of what is meant by the natural order of causes and effects. Indeed, one is tempted to say that he had a better notion of this order than David Hume, notwithstanding the fact that the acute analysis of the latter stripped away the mystical envelope that had before shrouded the conception of the causal relation, and laid bare the fact that causality means antecedence and succession, and is not a something in its nature obscure and incomprehensible. It seems rather natural for men, when an accession of light reveals some defect in a conception in which their mind has heretofore rested content, to go further in their reconstruction than they are justified in going. In their desire to reject error they not in-

[1] 'Méditation Troisième.'

frequently throw away with it more or less truth. To Hume the cause and effect relation becomes one of mere antecedence and consequence, with an undue emphasis upon the 'mere'; and he finds it in no sense 'necessary' that effects should follow from their causes. In following his argument we are everywhere struck with the fact that the world as an orderly system, a mechanism subject to invariable law, disappears from our view, and our attention is occupied instead with certain subjective conceptions which concern rather the psychologist than the student of physical science—such conceptions as custom, habit, expectation of the constancy of nature, which last Hume discovers to be a tendency for which no reason can be given.[1]

The reader of Hume feels the solid world crumble away beneath his feet. He has been accustomed to think that only his own ignorance stands in the way of his discovering why a particular effect follows from its cause. He now learns that the fact must be accepted blindly. Certain phenomena in our experience follow certain others, or, at least, have followed them, and we expect this to happen again. This expectation is something for which no reason can be given; and yet he cannot help feeling that it is highly desirable that some reason should be forthcoming. The relation of cause and effect appears to be arbitrary and incomprehensible. He has, furthermore, been accustomed to believe in natural necessity as something very real, and it has been to him a guarantee of the stability of the order of nature. He is now told that the connection of causes and their effects is in no sense necessary. Only a Hume can live contentedly in a world which thus resolves itself into mist and leaves one suspended in the void.

The error which lies at the root of such misconceptions is identical with that upon which I have commented a little above. It is the error of incomplete analysis. Just as the disinclination to use the word 'cause' in giving an account of the order of

[1] 'Inquiry Concerning the Human Understanding,' §§ 4–8. This is, I think, a fair characterization of the spirit of the Humian doctrine. It would, of course, be unjust to maintain that Hume recognized no distinction between antecedents in general and antecedents that may be regarded as the causes of particular occurrences. See his 'Rules by which to judge of Causes and Effects,' 'Treatise of Human Nature,' Part III.

nature has its origin in an imperfect apprehension of the fact that the notions of cause and activity are quite distinct from each other, and that there is no reason why, under certain circumstances, the one should not be taken and the other left, so the notion that the connection of causes and effects is a something inexplicable, which must simply be accepted without hope of explanation, arises from an imperfect apprehension of what science has done and is doing in the discovery of the causal relations of things, and also in a misconception of what the word *explanation* properly means. In the mechanical system of things of which I have been speaking in this paper it is probable that no change whatever can take place the total antecedent of which can be sought in only a part of the system. In other words, the whole condition of things at any given instant is due to the whole condition of things at the instant preceding. Nevertheless, science is not shut up to a knowledge of causes so broad and so vague as this. It is quite possible to pick out with some degree of accuracy individual antecedents and relate them to individual consequents, recognizing the fact that, although other antecedents may not have been without their influence in the result, nevertheless this influence is a negligible quantity and may without inconvenience be overlooked. Were it not thus possible to isolate certain factors in the total antecedent and connect them with certain factors in the total consequent, the science of mechanics could never have arisen. The method by which it arrives at its results has been clearly described,[1] and it is unnecessary for me to enter into the subject here. It is, however, important to note that this singling out of particular antecedents and connecting them with particular consequents gives rise, in the mind even of the student of mechanics, to a distinction between antecedents in general and that particular antecedent which, in a given instance, he connects with a particular consequent. Perhaps I should say keeps alive the distinction, for it exists in common thought as well as in science; the difference being that, whereas the student of science recognizes the fact that many antecedents not obtrusively present may contribute to a given result, and realizes the

[1] *E.g.*, Pearson, 'The Grammar of Science,' Chap. VIII.

difficulty of disentangling the threads in the skein, the plain man usually has his attention so taken up with the one thing that he settles upon as the cause of a given phenomenon that he quite overlooks the many concurrent causes which are less prominent or less interesting. In either case there is a recognition of the distinction between a 'mere' antecedent and that particular antecedent which is recognized as a cause. The obliteration of this distinction cannot but result in confusion. It is, consequently, with a good deal of satisfaction that the reader of Hume turns over the pages of the text-books on logic to discover that not every antecedent is a cause, but only a 'necessary' or 'indispensable' antecedent. It seems, then, that it is misleading to speak of the relation of cause and effect as that of *mere* antecedence and succession. There appear to be antecedents and antecedents; those which are, in a given instance, to be taken as causes, and those which are not.

III.

But what is a 'necessary' or 'indispensable' antecedent? Has not Hume pointed out that the relation between a cause and its effect is in no sense necessary, and that it is quite conceivable that the natural order of things should not be what it is? This latter question one may answer by showing that Hume wrote as he did because, while be recognized a distinction between two conceptions, he did not recognize it with perfect clearness. He saw that an effect is not contained in its cause as the conclusion of a syllogism is contained in its premises. Natural necessity, he saw, was not logical or mathematical necessity, and seeing this he felt impelled to deny that there is such a thing as natural necessity at all. In this he was wrong. The word necessity has, and has had for centuries past, two distinct meanings, and no man has a right to throw away one of them merely because it is not the other. To show that a given antecedent is a 'necessary' antecedent or cause, it is not necessary to show that the consequent is logically contained in it and cannot be denied without self-contradiction. It is only necessary to turn to the inductive logic and see whether there is

good reason to believe that the more or less complete elimination of other antecedents will leave this relation of antecedent and consequent virtually intact. The necessity of nature is but another name for the orderliness to be discovered in the system of things, and it is a total repudiation both of the knowledge of things which obtains in common life and of the more exact knowledge characteristic of science, to maintain that we cannot attain to a more or less detailed acquaintance with this world-order. It is not the duty of the metaphysician to show what antecedents are 'necessary' or 'indispensable.' It is the duty of the investigator of nature; and he can fulfil this duty perfectly well without paying the least attention to those mystical notions of causality which have in the past introduced a needless obscurity into human thought.

The relation between cause and effect is, therefore, a necessary one, in an intelligible sense of the word, and the denial of this necessity can only result in shaking that wholesome confidence in the order of nature possessed in some degree by the unlearned and in a higher degree by those whose knowledge of nature is more exact and extended. Sometimes this denial proceeds from a desire to remove that feeling of apprehension and even repulsion which arises in many minds at the thought of this gigantic mechanism which seems to sweep through its series of successive conditions with the impassivity of fate—a world in which even a sparrow cannot fall to the ground except according to law, but one in which the dance of an atom, the fall of a sparrow, the death-struggle of a man, appear to have one and the same significance, and to be summed up in those more or less complicated formulæ which describe the motions of material particles with reference to each other. Even so keen a man as Professor Huxley tries this method of soothing the anxieties of those who contemplate such a world with discontent, and suggests that if we will try to eliminate from our thoughts of the order of nature the notion of necessity, and will bear in mind that we are dealing with the mere relation of antecedence and consequence, we shall feel rather better.[1]

It is quite true that to the unreflective there may seem to be

[1] 'Methods and Results,' N. Y., 1893: 'On the Physical Basis of Life.'

something less august and inevitable in the succession of changes which take place in the material world when one has denied necessity to nature and has elected to regard what takes place before one's eyes as a mere play of antecedents and consequents. The starch appears to be taken out of the fabric; it hangs more limp and diaphanous. And yet, what has one gained? The pattern is precisely what it was. If it was ugly then, it is ugly now. Figure succeeds figure in the same inevitable order, and he who had reason for complaint before, has lost none by the change. The word *necessity* he has found unpleasant, and someone has obligingly given the thing a new name. Even so may the trembling householder decide to call the midnight marauder a visitor, and feel reassured and comforted. Meanwhile the man has suffered a real loss. He has lost sight of a useful distinction, and the order of nature has come to seem to him less stable and dependable than it was before.

It is, then, through an incomplete apprehension of what is properly meant by natural necessity that one is lead to deny necessity to the relation of cause and effect. And it is through a misapprehension of what is meant by *explanation*, that one is led to maintain that it is impossible to explain why certain causes should be followed by certain effects.[1] It was remarked

[1] I know no better illustration of the Humian exenteration of the notion of causality than that presented in the fourth chapter of Professor Pearson's *Grammar of Science*. He discards the idea that a cause is the occult and mysterious thing that has sometimes passed by that name. He agrees with Mill in thinking that causation is 'uniform antecedence.' But he finds it necessary to insist that the relation of cause and effect is not a *necessary* one (2d ed., pp. 113, 116, 118, 119), and he reiterates the statement that science, in discovering causes and effects, does not *explain* things': "Mechanical science no more explains or accounts for the motions of a molecule or of a planet than biological science accounts for the growth of a cell" (p. 115); "in no single case have we discovered *why* it is that these motions are taking place; science describes how they take place, but the *why* remains a mystery" (p. 120); "when we say that we have reached a 'mechanical explanation' of any group of phenomena, we only mean that we have described in the concise language of mechanics a certain routine of perceptions. We are neither able to explain why sense impressions have a definite sequence, nor to assert that there is really an element of necessity in the phenomena" (p. 116). It seems odd that Professor Pearson did not see that, if science (in the broad sense of the word) had really succeeded in finishing her task, there ought to be no *why* and no *mystery*. They disappear by absorption into the *how*.

by Immanuel Kant that it requires some sagacity for a man to know what questions he may safely ask. The remark was a wise one. There is a sense in which it is proper to ask for the explanation of this or that occurrence, and there is a sense in which it is not. Both in common life and in science we are constantly seeking an explanation of what comes to pass, and are constantly finding certain explanations satisfactory. The fall of the apple to the earth, the motion of the moon in its orbit, the ebb and flow of the tides, all these we regard as explained when they are seen to be illustrations of the laws of mechanics. The particular occurrence in question is found to have its appropriate place in the mechanical world-order, and we should rest content with this, for this is explanation. But if we will go on to insist that the whole mechanical system is a something to be accepted as inexplicable fact, we deserve any unhappiness that such reflections may occasion us. We extend the meaning of the word explanation quite beyond what is legitimate either in common thought or in science, and then complain that we lack an explanation of something, sadly electing to regard this something as 'brute fact.' This is not a recognition of the truth that no explanation can sensibly be asked for; it is a foolish insistence upon the fact that none is forthcoming, and, of course, carries with it the suggestion that it would be highly desirable if one *were* forthcoming. 'Brute fact' means fact that stands in need of explanation and appears to lack it. To call the system of things as a whole 'brute fact' is simply misleading.

IV.

Such reflections as the above should, I think, serve to set aside certain of the objections which some may be inclined to urge against the world as mechanism. If the conception of mechanism seems to us absurd, it is because we imperfectly comprehend what that conception is, as it is gradually growing clearer to science. If we deny the existence of material causes, it is because we confound the notions of causality and activity, or erroneously assume that a cause can only be something occult and mysterious, which must be eschewed by science.

If we repudiate natural necessity, it is because we fail to perceive that the word 'necessity' is an ambiguous one. If we insist that science cannot offer an explanation of the occurrences in the material world, it is because we give the word 'explanation' an unjustifiable meaning.

It is, however, quite possible for one to avoid these errors and yet to feel very dubious about yielding assent to the doctrine that the world of matter is a perfect and independent mechanism, every change in every part of which must find its whole explanation in the system itself. There are those whose mental vision can trace the paths along which the primitive elements of the universe have travelled out of their inconceivable abysses, when they came together to form those aggregates of matter which ultimately became worlds, and can find them written in the book of what was to be according to invariable mechanical law. They can see, with the eye of faith, the results of mechanical causes in the birth of the chemical elements, and the emergence of the new kinds of matter which result from their combinations. If they cannot prove that they are here in the presence of a veiled mechanism, they at least do not find it hard to believe it, and they can indulge a more or less lively hope that it may some day be granted them to have a glimpse behind the veil. But when they pass from the inorganic to the organic world, and stand in the presence of life, they find it difficult to believe that an increase of knowledge will show that here, too, the conception of mechanism must reign supreme.

We are all impressed by the striking contrast between the living and what is recognized as mechanical. The word 'machine' calls before our mind a steam-engine, a spinning-jenny, or a printing press; a gross clattering mass of metal, between which and a rose or a violet the difference seems to be worldwide. The machine obeys laws clearly seen to be mechanical, it is comparatively simple, it appears adapted to the attainment of a particular end, but is incapable of attaining it by any but the one direct path along which we have set it moving. The plant presents the phenomena of life; which means the direct opposite of all this. Into the indefinite complexity of its structure we have no means of seeing clearly; its growth and

development cannot be shown to be the result of mechanical causes exclusively; it appears to move toward an end of its own, and to have a capacity for attaining this end by certain by-paths when for some reason the direct road is obstructed. The plant develops according to a certain plan, and after this plan reproduces its kind. When the end of a branch is pruned away, buds form and new sprouts make their appearance to carry out the idea with which the mutilation interfered. If we have here a machine, it is at least a machine which must not be brought down to the level of the mechanisms constructed by man to carry out his purposes. And if we pass from plant to animal the contrast is, if possible, more striking. I have said above that, in the mechanical view of the material world, the boy who strikes a dog with a stick, and the dog that receives the blow, are simply masses of matter undergoing certain changes in their space-relations to one another, all of which changes are explicable by the laws of mechanics, and form an inevitable succession of states related to each other as cause and effect. Yet the fact remains that a boy whom we recognize to be of a certain stamp will, as we know before the act, hit the dog under the most varying circumstances—whether the animal be on this side of him or on that, within easy reach of him or further away, standing still or moving. He will even chase him around the house again and again; in which case the description of the successive positions of the material particles which make up boy, stick and dog, in their relations to each other and to other things, must attain to enormous complexity. The one certain thing, in the present incomplete state of our knowledge, seems to be that the boy will hit the dog—*i. e.*, that, to speak mechanically, a certain final collocation of material particles will be attained. The path by which it is to be attained seems highly uncertain.

If, then, this boy and this dog are machines, they certainly differ widely from the machines which are commonly recognized as such, and it is manifestly an error to overlook the difference. It is possible to be so impressed by it as to maintain that the notion of mechanism must be abandoned altogether when one is considering such things, and with it abandoned the explana-

tion by a reference to efficient causes which is the very sheet-anchor of science. On the other hand, one may estimate this difference at its full value, and nevertheless believe that the phenomena presented by living beings, growth, development, reproduction, activities of the most varied description, dissolution,—all would be capable of description in mechanical terms, were our knowledge and our intellectual powers sufficiently advanced. One may point out that the possibility of a detailed description of the processes by means of which things come about is not in the least incompatible with the recognition of the fact that such and such things do come about. In other words, one may point out that the existence of *efficient* causes—the 'necessary antecedents' of which I have spoken above—is not incompatible with that of *final* causes, for these latter are only the ends which are attained through the instrumentality of the former.

It is a matter of common experience that it is quite possible to have a knowledge that such and such an occurrence will take place, and yet to be in the dark as to the series of causes which will bring it about. One may know that it is likely to rain, and yet have the vaguest possible notion of those atmospheric changes which give birth to the falling drops. Similarly, the simultaneous appearance of boy and dog within one's horizon may give rise to the conviction that sooner or later these two masses of matter will stand in the definite mutual relation referred to above; and yet one may have no clear idea of the particular series of changes which will precede this particular result. Thus one may know empirically that with one's gun at a certain elevation, with a given charge of powder, and with a given projectile, one may hit a target at a fixed distance. At the same time one may be quite unable to calculate the path of the projectile from the gun to the target. When one knows something of the science of mechanics, one no longer thinks of the beginning and end of this series of changes as constituting all that is worthy of attention in the occurrence as a whole. There are no longer one cause and one effect; there is an indefinite series of causes each followed by its effect, and the initial antecedent is no more important to the final result than

are any of the others. Those who incline to view the universe of matter as a perfect mechanism must look upon the series of changes which take place in the relative positions of the boy and the dog as constituting such a chain of causes and effects. They cannot admit for a moment that the end is fixed independently of the means. To them the end is simply one term in a complicated series, and its coming into existence is conditioned upon the links in the chain preceding it. But they may freely admit that they are sometimes pretty sure of the end when they are by no means clear as to the exact path by which it will be attained, as has been said above. They may point out that we can be very sure when we drop a ball inside of the rim of a bowl on the table before us that the ball will ultimately come to rest at the very bottom of the bowl, and yet we may find it difficult or even impossible to describe in detail all the motions of the ball before it comes to rest. Which means that in a causal series admittedly mechanical it may be possible to predict the appearance of a given term, even when we have no definite knowledge of those that precede it.

To all this it may be objected that it is easy to suggest that all the changes which take place in those masses of matter that we call living beings may find their explanation within the realm of mechanics, but it is another thing to prove that they actually do this. When the boy's gaze has once rested upon the dog, the end seems to be fixed, as in the ancient conceptions of fate, and the means appear to be conditioned by the end, not the end by the means. Can a mechanism select this and reject that, taking what serves a given end and refusing what does not? Has anyone the least conception of a mechanism that can pick and choose in this way? If not, why insist that living beings must be brought under the conception of mechanism?

To this one may answer that, even in the gross mechanisms constructed by man, we are not without some suggestion of selection. To get the bit of chocolate out of the metal case that stands against the wall in the railway station, one must drop the appropriate coin into the slot, just as one must deposit the appropriate coin in order to obtain a sandwich from the

woman at the lunch-counter. And one wholly ignorant of the extent to which the construction of mechanisms has been carried, might easily be tempted to think that the motions of the machine that tests the weight of the coins committed to it, sorting out into different heaps the perfect and the imperfect, are determined by the end to be attained and not by a chain of mechanical causes. To one who understands the construction of such mechanisms there is nothing marvellous in the thought that a definite end will be attained as the result of a strictly mechanical series of processes, and that the attainment of other results will be provided against just because of this series of causes. Between the most ingenious of such machines and the boy of whom I have been speaking, there is doubtless an enormous difference, and one which it would be foolish to overlook. But it should not be forgotten that between the human body and organic structures which are less highly developed there are also differences which are sufficiently striking. We are not compelled to pass at a jump from a weighing-machine to a man. There are forms of life that exhibit phenomena which, if they do not serve to bridge the gulf between the organic and the inorganic, at least bring us to the brink with a strong disposition to launch away. The evidences of what we are inclined to recognize as choice, in an unequivocal sense of that word, grow less and less as one descends in the scale, and the approach to mechanism, as we commonly think of it, seems a sufficiently close one. If we elect to believe that all motions in matter cannot be accounted for by a reference to mechanical causes, where shall we make the break? Shall it be between the organic and the inorganic, or shall it be placed somewhere above this point? The question is not an absurd one, for, as the student of the history of philosophy well knows, thoughtful men have not been at one touching the answer that should be given to it. The disciples of Descartes drew the line between man and all that lay below him. This would make the boy of our illustration something more than a mechanism, but the dog, who appears equally active, and almost equally ingenious, would be a mechanism and nothing more. Modern science, imbued as it is with a strong desire to remove what

seem to be breaks in the orderly development of nature, would find it difficult, having gone as far as this, not to go farther.

The adherent of the view that the material world is through and through a mechanism may argue that the objection which has been urged to his view is, in so far as it really is an objection, nothing more than an *argumentum ad ignorantiam.* If it be merely intended to point out that, on the slender basis of actual knowledge which we at present possess, modesty is an appropriate virtue, and dogmatism a thing to be deplored, even the most enthusiastic student of science should welcome the admonition. It is foolish to maintain that we know, where we only have hints and guesses. It is, of course, also foolish to reject those hints and guesses, if they are the best that we have at the present moment. One should take them at what they are worth, holding one's opinion tentatively, and striving neither to be blinded to new light by ancient prejudices, nor carried off of one's feet by the currents of contemporary thought, which may or may not happen to be setting in the direction of true progress. If, again, the objector merely wishes to emphasize the fact that boys are not such machines as we place in position against the wall of a railway station, and to insist upon the truth that there is in our experience such a thing as the choice of ends and the adjustment of means to their attainment, no sensible man can have any quarrel with him for this. There can be no more serious error than to suppose that because all the changes which take place in a boy's body, and in its relations to other things, can be brought under the conception of mechanism, therefore, the boy must no longer be regarded as a boy, but rather as a bit of furniture. As well argue that because a boy is an animal we must look upon him as a flea. When things widely diverse are brought under the same general concept, it does not mean that the differences that distinguish them are obliterated. It is, therefore, of the utmost importance to remember that an extension of the concept of mechanism does not in the least wipe out the distinction between what are commonly recognized as machines, and living organisms. That distinction is a marked one, and one must be a slave to one's idea when one is misled into overlooking it. To call attention to the distinction,

where there is danger that it may be forgotten, is a public service.

But if the objector does not intend to do either of the things mentioned just above, and does intend dogmatically to maintain that no extension of our knowledge of boy, dog, stick, and their material environment—not even the knowledge of which at present science dreams and which it recognizes as quite beyond its grasp—would reveal that the series of changes which have taken place are part of a mechanical order of things, he seems to arrogate to himself an authority to which he can lay no just claim. Were he in a position to show that the attainment of such and such ends could not be effected by a series of mechanical causes, his position would be a reasonable one. As he is only in a position to show that no one knows just how it can be, it does not appear very reasonable.

It does not seem, then, that we need be deterred from assuming, as a working hypothesis, at least, that the universe of matter is a perfect mechanism, either by supposed difficulties connected with the concept of mechanism itself, or by the fact that science is not now in a position to prove the justice of all its guesses at the truth. But there is one objection which appears to have more weight. In our common experience of the world, it is an undeniable fact that there are such things as *minds*. It is as fair to ask what these are, and what is their true place in a reasonable scheme of the system of things, as it is to ask any of the questions touching the nature of matter with which the student of physical science occupies himself. For an answer to such questions one can no more turn directly to the crude and undigested experience of the plain man, than one can for an answer to questions concerning the nature of matter. Still, there is a way of approaching such questions. And if it be discovered that a given view of the physical universe is really incompatible with what seems, after critical examination, to be known about minds, it is an argument against that view not to be despised.

PRACTICE AND ITS EFFECTS ON THE PERCEPTION OF ILLUSIONS.

BY PROFESSOR CHARLES H. JUDD,
University of Cincinnati.

The results to be reported in this paper were obtained by two observers in extended series of measurements of the Müller-Lyer illusion. The series of measurements were undertaken for the purpose of determining quantitatively the results of practice on the strength of the illusion. The results here presented do not answer by any means all the questions that arose during the investigation, but the method proved to be so well adapted to the investigation of the effects of practice, and the results are so suggestive because of the light which they throw on the nature of the illusion, that the writer has felt justified in presenting this account of the preliminary experiments already performed, and in drawing some of the inferences which seem to issue from the data gathered.

The method was the well-known method of determining quantitatively the strength of the Müller-Lyer illusion. The figure with the oblique lines turned inward was drawn at the edge of a card, and this was placed over a second card on which was drawn a line of indefinite length having at its uncovered extremity the oblique lines turned outward. The two figures of the illusion were thus obtained in horizontal juxtaposition and in such relation that the overestimated figure on the lower card could be adjusted by the observer until it seemed equal to the upper figure with its underestimated line.

A number of different figures were used and will be designated in the following discussions by the length of the standard line, which is in each case the line in the underestimated figure, and by the angle between the oblique lines. Thus, 'figure 54 mm., 90°', indicates that the standard is 54 mm. in length and

that the oblique lines in the figure form angles of 90 degrees with each other. At no time did the observers have any objective means of determining the true relations of the lines. The length of the adjusted line was carefully marked off at each trial on a slip of paper and filed for measurement. The observations were made in series of from 20 to 200 at a single sitting, the sittings covering periods of one half an hour to two hours. Generally, several series were made on the same day.

One of the observers was the writer. He expected the illusion to grow weaker with practice and at the 500th test and again at the 900th he measured the lines to discover how much further the experiment was to be carried. His periods of work, as will be seen from the table and curves, were somewhat irregular.

The second subject E was entirely ignorant of the character of the results throughout the whole course of the experiment. The only expectation of subject E was that the results would grow somewhat more regular. At no time was there any suspicion that the illusion was changing in strength. Subject E was also much more regular in periods of experimentation and in mode of adjusting the figure. During the first practice series performed by E, the figures were always held in the horizontal position, the underestimated figure was always held on the right-hand side, and the overestimated line was always set too long at the beginning of the experiment and gradually shortened until the desired length was reached.

The two observers represent, accordingly, two different kinds of training. Subject J was trained somewhat more irregularly and with a background of abstract knowledge and expectation accompanying the perceptual practice. The subject E was trained in a very regular mode of operation and in a purely perceptual way, without any accompanying biasing knowledge or expectation.

Before entering upon the practice series both subjects made 25 determinations with each of the figures to be used by him. These results are marked in the figures, 'Illusion before practice.'

We turn now to the detailed results. Subject J began the experiment by making 25 determinations each on figures 54 mm., 90°; 54 mm., 45°; and 68 mm., 90°. He then began continuous experiments with figure 54 mm., 90°. In this practice series the figures were held in a horizontal position and the standard figure was uniformly on the right-hand side, but the mode of adjusting the figure was irregular, sometimes starting with the compared line too long, sometimes starting with it too short. The results are presented in Table I. and in Fig. 1. Each entry in the second column of the table, except in a few cases as indicated in column one, and each point in the figure, is the result of averaging twenty consecutive measurements. One horizontal line in the table, and one vertical line in the figure, indicates a pause of an hour or more during a given day. Two lines indicate the pause of a night. The third column in the table presents the mean variation, and the fourth and fifth columns contain respectively the highest and lowest single determinations in the given group of twenty. Full tables will not be given of the other series. The chief results can be exhibited by the curves, and it may be stated here that the results in regard to variation are fully represented by this first case.

In general it may be said that each period of practice results in some improvement on the part of the observer. The column of mean variation indicates also in general in each series—at least until the subject reaches the maximum of practice—an increasing regularity of perception. One especially interesting phase of the curve is the sudden improvement which showed itself after the pause of two days between measurements 760 and 761. There is no special ground for this sudden change. Nothing parallel to it occurred in the case of the other observer, E. It is probably due to the fact, so well known in practical experience, that a pause is sometimes beneficial in the pursuit of any kind of training.

The most important fact is of course the final result. The illusion disappears after practice. It disappears, not by any process of judgment or any process of indirect correction. The line comes to look differently than it did at first. A most strik-

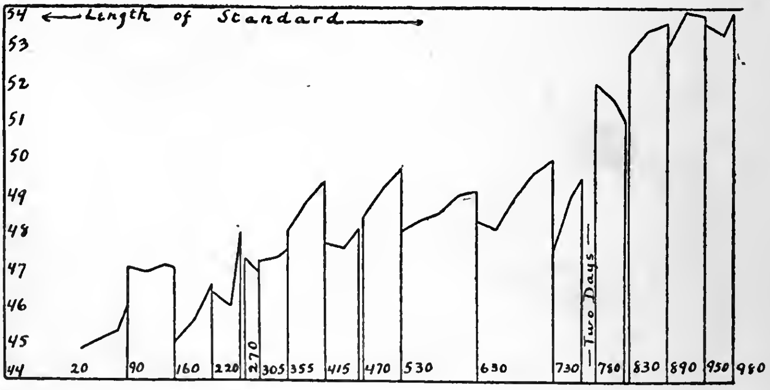
Table I.

SUBJECT J.

Fig. 54 mm., 90°. Position Horizontal with Standard on Right Side.

No. of Measurements.	Avg. Illusion.	M. V.	Highest Determinat.	Lowest Deter.
1–20	44.8	0.83	48.0	43.0
21–40	45.1	0.50	46.5	44.0
41–60	45.3	0.87	48.5	43.0
61–70	46.0	0.80	48.5	45.0
71–90	47.0	1.38	49.0	42.5
91–110	46.9	0.79	49.0	45.0
111–130	47.1	0.56	48.0	46.0
131–140	47.1	0.75	49.0	46.0
141–160	45.0	0.88	46.5	43.0
161–180	45.6	0.70	48.0	44.0
181–200	46.6	0.67	48.0	45.5
201–220	46.4	1.03	48.5	44.0
221–240	46.0	1.03	48.0	43.5
241–250	48.0	0.85	49.5	46.5
251–270	47.3	0.95	49.0	45.0
271–285	46.9	1.00	49.5	44.5
286–305	47.2	1.07	49.0	45.0
306–325	47.2	0.85	49.5	46.0
326–335	47.5	0.70	48.5	46.0
336–355	48.0	0.83	49.5	46.0
356–375	48.8	1.07	51.0	46.0
376–395	49.3	0.60	50.0	48.0
396–415	47.7	0.84	49.5	46.5
416–435	47.6	0.84	50.0	45.5
436–450	48.1	0.50	49.5	46.5
451–470	48.4	0.85	50.0	46.0
471–490	49.3	0.81	51.5	48.0
491–510	49.7	0.75	51.0	48.0
511–530	48.1	0.80	49.5	46.0
531–550	48.3	0.77	50.0	46.0
551–570	48.5	0.83	50.0	47.0
571–590	49.0	0.38	50.0	48.0
591–610	49.1	1.04	50.5	47.5
611–630	48.3	0.92	51.5	45.5
631–650	48.1	0.74	50.0	46.0
651–670	48.9	0.80	51.0	47.0
671–690	49.6	0.74	51.0	47.5
691–710	50.0	0.75	51.5	48.0
711–730	47.5	1.18	50.0	45.0
731–750	49.0	0.70	51.0	47.0
751–760	49.4	0.50	50.5	48.5
Two Days' Pause.				
761–780	52.1	0.53	53.0	51.0
781–800	51.6	0.55	53.0	51.0
801–810	51.0	0.20	52.0	50.0
811–830	52.7	0.87	54.5	50.0
831–850	53.4	0.46	54.0	52.5
851–870	53.6	0.49	54.5	53.0
871–890	53.0	0.73	54.5	51.5
891–910	53.9	0.57	55.0	53.0
911–930	53.8	0.81	56.0	52.0
931–950	53.6	0.56	54.5	52.0
951–970	53.2	0.63	54.0	51.5
971–980	53.8	0.39	54.5	53.0

Fig. 1.



ing exhibition of the change which has taken place can be seen by setting the line, after practice is complete, at the length at which the records show that one set it early in the experiment. It seems so strikingly and so clearly too short to the now trained eye that it is almost unbelievable that the illusion could ever have been so strong. We have here, then, a change in the perceptual process, which change has taken place gradually through repeated efforts to deal directly with the object perceived.

One might be tempted to hold that the change was not a purely perceptual change, uninfluenced by expectation, in the case of observer J. It is just at this point that the curve of subject E (Fig. 2) is of interest as a curve of similar import, and, what is more, as a curve of similar length to that of J. Subject E, it will be recalled, had no biasing judgments. The result was in this case due to practice in comparing the lines, and to this practice only.

FIG. 2.

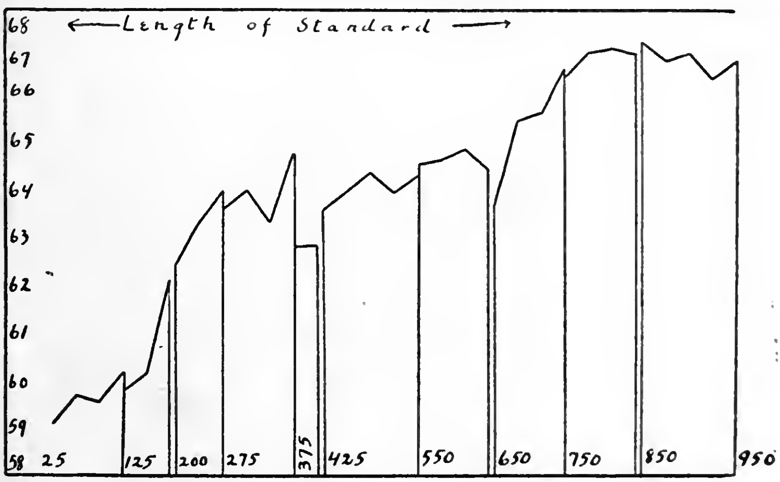


Fig. 2, it should be noted, represents at each point an average of twenty-five measurements. It also shows a more regular development—a fact which is doubtless related to the regularity of E's practice.

Taken together these two series of perceptual changes show clearly that judgment and perception are different kinds of

processes. Judgment cannot do for the observer what must be accomplished by practice. Abstract ideas cannot take the place of direct perceptual experiences. Furthermore, the change which took place in the perceptual process did not reveal itself to introspection. The very mode of conscious activity underwent a change. The observer came to stand on a different level of perception after practice, but the process by which the change of level had been effected can only be inferred; it could not be observed even when the observer was looking forward, as was observer J, to the change.

The significance of these conclusions for the general genetic study of mental life cannot be overlooked. If it is true that in a limited series of experiences a change so marked as this may take place without leaving in direct consciousness a single trace of the process by which the change was effected, then certainly we must hesitate in adopting any explanation of mental development which is based on the assumption that the later stages of mental life are different from the earlier stages only in a quantitative way. The whole genetic problem is seen to be a problem requiring methods which reach beyond introspection, and requiring, furthermore, a careful scrutiny of the qualitative aspects of the changes effected through development.

To return, however, to the experimental results. After the observers had gained their experience with their practice series, a number of additional series were tried. The additional series tried by J may be presented first.

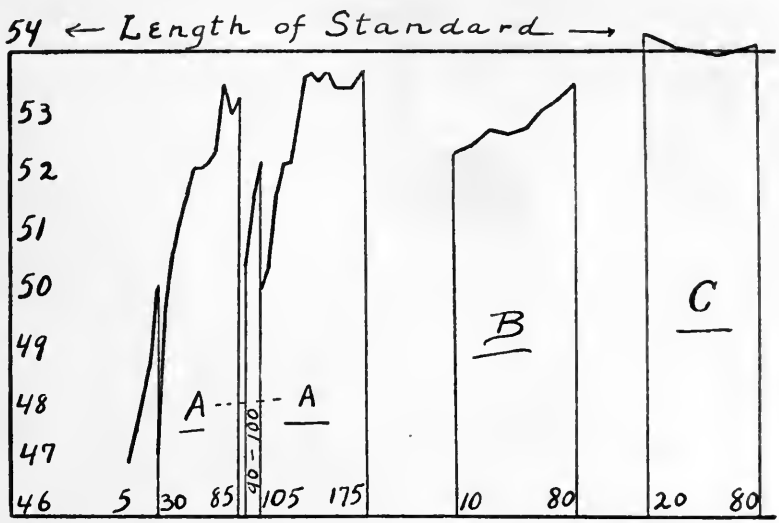
Before practice J had made twenty-five determinations, each with figures 54 mm., 45°; and 68 mm., 90°. The average illusion for figure 54 mm., 45°, before practice was (in terms of the length of the adjusted line) 45.1 mm. with a mean variation of 1.73 mm. After the practice series with 54 mm., 90°, had been completed, twenty-five new determinations were made with 54 mm., 45°. The position, it should be noted, in all these experiments was the same, namely, horizontal, with standard figure on the right-hand side. After practice with the 90° figure, the illusion had nearly disappeared for the 45° figure, though there had been no practice with the latter. The average illusion (again in terms of the adjusted line) was now 53.0

mm., with a mean variation of 1.06 mm. In other words, the effects of practice had been transferred in a very marked degree to the 45° figure, with which there had been no practice.

The results for figure 68 mm., 90°, before and after the practice series with figure 54 mm., 90°, were as follows: before practice 58.7 mm., with a mean variation of 0.95 mm.; after practice 67.3 mm., with a mean variation of 0.95 mm.

If now the character of the figure was changed by turning it around so that the standard line was on the left-hand side, the transfer of practice seemed to be much less direct. Fig. 3,

FIG. 3.

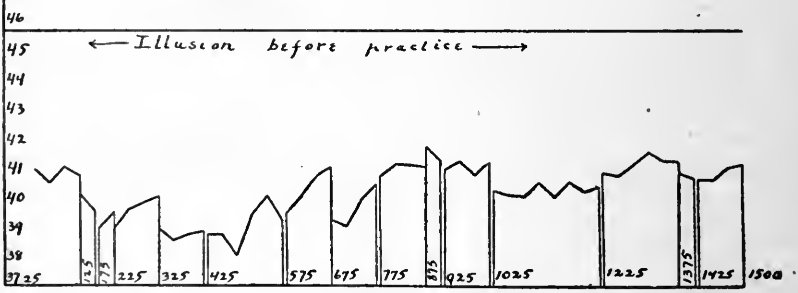


A, shows the results of 175 such determinations with 54 mm., 90°, inverted (each point representing an average of five determinations). This whole series was made after the practice series, but it will be noticed that at the outset the illusion is almost as strong as in the first determinations of the practice series. From this beginning on, however, the curve rises very rapidly, indicating clearly the transfer of practice. Fig. 3, *B*, shows the results obtained by turning the figure 54 mm., 90°, into a vertical position with the standard line above. Similar results were obtained for the vertical position with the standard below. Finally, instead of comparing

the underestimated Müller-Lyer figure with the overestimated figure of this illusion, a comparison was made between the standard 54 mm., 90°, and a simple straight line placed somewhat below and to the left. The result is given in Fig. 3, *C*.

In all these cases there is an obvious transfer of practice, in spite of the varying conditions. In view of the special conditions which seemed to surround the inversion of the figure, as shown in the results reported in Fig. 3, *A*, observer E was especially prepared for a study of that particular phase of the effect of practice. The practice figure of E was, it will be remembered, 67 mm., 45° (see Fig. 2). Before undertaking this practice series E was tested 25 times on figure 53 mm., 90°. This latter figure was, however, in these preliminary and in certain of the latter tests, placed with the standard on the left, instead of on the right-hand side. The result of this preliminary series is represented by the line marked 'before practice,' in Fig. 4. Expressed in numbers the illusion before practice was

FIG. 4.



45.6 mm., with a mean variation of 0.86, the standard figure being 53 mm., 90°.

In order to test E for the easy form of transfer of practice which J had shown when figures of different size were presented

in the familiar position of the practice series, that is, with the standard on the right side, E was given a special series of 25 determinations with figure 53 mm., 90°, with the standard on the right, after completing 750 measurements in the practice series. The result is given in Fig. 4 in the line marked 'Improvement after 750 trials of 68, 45°.' Expressed in numbers the average illusion was at that time 50.4 mm., with a mean variation of 1.3 mm., or an improvement of 4.8 mm.

After the whole practice series was completed, the observer E was given the figure 53 mm., 90°, in the position in which the first 25 determinations had been made, *i. e.*, with standard on the left. It should be recalled that E did not know anything about the disappearance of the illusion in the practice series. No information was given in regard to the change in figure or the difference in position of the standard line. No particular curiosity was aroused by any of these changes in the routine of the experiment. So far as the observer was concerned the problem was merely to set the two lines so that they seemed equal and to make the judgments as regular as possible. Indeed, the subject expressed a preference for figure 53 mm., 90°, because it was shorter and because the oblique lines were further apart, thus making perception and comparison apparently easier. The results of 1,500 measurements are reported in the lower curve in Fig. 4. The mean variations, which are not expressed here, never exceeded 1.27 mm., and were for the most part below 0.90 mm.

The results of this last series are by no means difficult to interpret. The practice which had been gained with figure 67 mm., 45°, with the standard line on the right-hand side, was transferred, but in a negative way, to the new figure 53 mm., 90°, with its standard line placed on the left. That is, in spite of a change in the length of the lines perceived, and in spite of a change in the degree of obliquity of the additional lines, and finally, in spite of a new arrangement of the figures, the effects of practice were obviously carried over to the new conditions. Furthermore, it is evident from the curve that the observer started on this second series of measurements with a thoroughly established habit of interpretation. The failure to improve

during the long series of 1,500 measurements brings this series into the sharpest contrast with the earlier practice series. The mental condition at the beginning of the practice series was one of uncertainty and more or less tentative effort to work out a new mode of interpretation. Gradually there grew up a mode of interpretation suited especially to the conditions presented. Once established, this new mode of interpretation took the place of tentative efforts and became the fixed mode of interpretation. The subject, who had cultivated in a purely empirical way this fixed mode of interpretation, misapplied it, to be sure, and by this misapplication of the mode of interpretation he increased the strength of the illusion under the new conditions. But misapplication though it is, it is nevertheless evidence of transition from an untrained, tentative interpretation to a form of interpretation which is highly developed and firmly fixed.

An interesting contrast appears when we compare E's results with the way in which observer J behaved under similar conditions. With J there is obviously some difficulty in fitting the cultivated mode of interpretation gained in the practice series to the new inverted position of the figure (see Fig. 3, *A*). The acquired habit of interpretation is, however, not carried over in a negative way to the new conditions. It is carried over after some effort and readjustment. This ability of J to accommodate himself to new conditions is undoubtedly a consequence of the less rigid and less purely empirical character of the original practice series. To express the matter in everyday phraseology, J had acquired his mode of interpretation through practice and through practice only, but when he had completed the practice he knew what the character of his acquired mode of interpretation was, and he knew its special character sufficiently to recognize that it must be readjusted in applying it to new conditions. He had in addition to practice what we may call control of his acquired habit.

There are a number of lines of discussion suggested by these results. As pointed out in the opening paragraph of the paper, the experiments have not been carried as far as they may be carried or as far as the writer hopes to carry them, but even in this incomplete state they throw much light on the ques-

tions that have recently been raised in regard to the nature of practice, especially by the papers of Professor Thorndike and Dr. Woodworth.[1] There can be no doubt that in the experiments here reported, a mere change in the length of lines, or even the more marked changes in obliquity of the added lines and in the position of the whole figure, were no hindrance to the transfer of the effects of practice. These changes are, as the writer understands the reports of the two investigators mentioned, fully as marked as many which according to their results hinder the transfer of practice. Perhaps the apparently contradictory conclusions could be reconciled by attention to the fact rendered so obvious by the last series of measurements made by E, namely, the fact that the absence of improvement in a given line of mental activity is often the strongest possible evidence of the transfer of practice. It is evidently quite unjustifiable to judge of the nature of practice by a mere comparison of an observer's results with objective standards. Practice can be discovered only by methods which show something of the course of mental development as a series of subjective and relative changes rather than as a series of objective and absolute improvements.

A second line of legitimate inference from the experiments reported, relates to the nature of the illusion. In the first place, the observer, as he takes up the illusion in the beginning, is obviously without any definite mode of interpretation which he can apply under these conditions. The first stages of his experience with this illusion can not be described merely by saying that he applies to the figure the ordinary mode of interpretation. The fact that change sets in so soon shows that the observer tries a variety of modes of perception tentatively. He depends, of course, in a vague general way on ordinary experience, but he is evidently more or less confused and distracted from the main lines of the figure by the additional lines. The process of development is a kind of progressive perceptual organization in which the distracting lines are mastered and finally interpreted as a part of the larger whole—the final complex process of perception being an advance upon ordinary ex-

[1] Psychological Review, Vol. VIII.

perience in that a new special mode of interpretation has been developed to fit the complex group of conditions here presented. When a mode of interpretation has been developed to fit one group of conditions, it shows its special character by not being suited to immediate transfer to other, different complexes of lines.

As already pointed out, introspection does not give a clue to the nature of the changes in the mode of perception. One notes at first that it is natural to try to neglect the oblique lines and concentrate on the main lines of the figure. Early in the practice series both observers noted the feeling of having succeeded in abstracting from the oblique lines. That they had not done so appears in the fact that the illusion continued in almost its full original strength. Later there was less conscious effort directed towards abstracting from the oblique lines; one came to feel a sort of general mastery of the whole figure. This later feeling is the one that came to be more and more clearly marked in the later stages of practice. So far as these observations indicate anything in regard to the later forms of perception, they show that organized perception does not consist in the rejection of factors in the field of vision, but rather in a proper synthesis of all the factors into a single whole. Lower and higher stages of perception are thus to be described, not in terms of the amount or kind of content included in the percepts, but rather in terms of the mode of organizing such content as is present.

An illusion may be defined on this basis as a case of confused perception in which there is no fully developed mode of synthesis, or, as in the case of E's second series, as a perception in which the mode of synthesis is inappropriate to the particular conditions presented.

Finally, a word may be added to the above general discussion in the effort to meet the question which naturally arises as to the nature and conditions of these changes in modes of perceptual synthesis. Here again we have reached one of the limitations of the present investigation. One observation was made in regard to E which may be suggestive. At first the eyes ran over the figures of the illusion in an irregular way.

As the practice series progressed, a perfectly definite habit of eye movement developed. The eyes fixated first the right-hand extremity of the right-hand figure. Then they moved to the left-hand extremity of the right figure which was of course also the right-hand extremity of the left figure. From this second point of fixation a second movement was made to the left-hand extremity of the left figure. Then in one long movement the eyes moved back to their first point of fixation at the right-hand extremity of the right figure. When, now, the observer took up the inverted figure in the second series, the same form of eye movement, consisting of two short movements to the left, followed by one long movement to the right, was regularly repeated. Because of the changed position of the figure, however, the two shorter movements and the subsequent long movement were now, with respect to the oblique lines, in the opposite relation. These facts in regard to E's eye movements were not paralleled by any observable regularity in J's movements.

These facts of movement are the only suggestions which can be offered at present by way of contribution to the explanation of the perceptual changes that take place. But together with the other results they serve to outline the genetic study of the illusion. It is to be hoped that a further elaboration of this method of investigation will give a more definite answer to the questions raised. The purpose of this paper will be accomplished if it serves to suggest a method of investigation and certain preliminary results on the general problem of the development of perceptual processes.

MENTAL IMAGERY OF STUDENTS.

A Summary of the Replies Given to Titchener's Questionary by 118 Juniors in Vassar College.

BY PROFESSOR F. C. FRENCH.

This paper is a summary of the replies given by the class in psychology at Vassar College to the 'Questionary upon Ideational Type' in Titchener's 'Experimental Psychology.'[1] By kind permission of author and publisher I was enabled to have the set of questions reprinted and distributed to the class. This questionary has at least one marked superiority over similar ones that have been used in the past. It offers a fair proportion of tests for all departments of sense. The author very justly says of it: "It is insofar an improvement upon Galton's paper as that the questions upon auditory, tactual, etc., imagery are drawn from definite situations, and not made a mere appendix to the visual portion of the enquiry."[2] While this questionary is doubtless open to criticism, and any individual investigator might suggest improvements from his own point of view, some sacrifice of individual preference will make for the general good. A large number of reports based upon the same set of questions will offer opportunities for statistical comparison that could not be obtained in any other way. It is with the hope that this may be but the first of many similar reports, that I have prepared this summary. I have chosen this particular set of questions not only because of its intrinsic merits, but because it is the questionary offered in the work which is likely to be for a considerable time to come the standard manual of experimental psychology, and it may be hoped, therefore,

[1] Student's Manual, p. 198.
[2] Instructor's Manual, p. 391.

that many other social groups will be tested by the same questions.

The questionary was given out last April to the young women of the Junior class of Vassar College. They were not of course experts in introspective investigation, but they had listened to some discussion of the subject in the lectures, had tried a number of general experiments of this sort in class, and had read the chapter on Imagination in James' Psychology which was the text-book in use. The one thing emphasized in giving out the questions was that the answers should be accurate reports of actual trials. No doubt in the nine or ten thousand answers given there are a considerable number of careless observations and inaccurate statements. But the general average of accuracy is, I believe, very high. Internal evidence fully justifies this belief. When a student answers the question whether she can recall the voice of some well-known person by saying she can for she can always recognize her acquaintances by the voice, we can be sure that she has made no real trial of her power to recall the voice, but is reporting merely what she thinks she can do. The cases that show such direct evidence of having made no actual experiment are very rare—less than a dozen, I should say, altogether. On the other hand the instances that show positive evidence of careful trial are very numerous. Two classes of such evidence are common surprise at failure to recall something, and discrimination in the intensity or vividness of similar images. An example of the first class would be the statement of one who says, "I am greatly surprised that after repeated trials I cannot recall hunger and thirst"; of the second class, "I can recall the taste of salt very distinctly, sugar only faintly and lemon juice not at all." All but five members of the class handed in a set of answers. One of the 119 papers received was discarded because of a marked disparity in age. The writer was a woman who had returned to complete her college course after more than twenty years' absence, and was at least twice the age of the other members of the class. The following results are therefore the summary of 118 replies all written by young women of the usual age of College Juniors.

TABLE SHOWING ACTUAL NUMBER AND PERCENTAGE OF YES AND NO ANSWERS.

	Actual No. Yes.	Actual No. No.	Percentage. Yes.	Percentage. No.
I. Think of a bunch of white rosebuds, lying among fern-leaves in a florist's box.				
a. Are the colors—the creamy white, the green, the shining white—quite distinct and natural?	118	0	100	0
b, 1. Do you see the flowers in a good light?	106	12	90	10
2. Is the image as bright as the objects would be if they lay on the table before you?	59	52	53	47
c, 1. Are the flowers, and leaves, and box, well defined and clear cut?	115	3	97.5	2.5
2. Can you see the whole group of objects together, or is one part distinctly outlined while the others are blurred?	58	43	57	43
d, 1. Can you call up the scent of the rosebuds?	103	15	87	13
2. Of the moist ferns?	65	53	55	45
3. Of the damp pasteboard?	75	43	64	36
e, 1. Can you feel the softness of the rose petals?	114	4	96.6	3.4
2. The roughness of the ferns?	106	12	90	10
3. The stiffness of the box?	106	11	91	9
f. Can you feel the coldness of the buds as you lay them against your cheek?	108	10	92	8
g, 1. Can you feel the prick of a thorn?	85	31	73	27
2. Can you see the drop of blood welling out upon your finger?	110	4	96.5	3.5
3. Can you feel the smart and soreness of the wound?	62	52	54	46
h. Can you call up the taste of				
1. Candied rose leaves?	36	70	34	66
2. Candied violets?	87	29	75	25

3. Salt?	104	14	88	12
4. Sugar?	103	14	88	12
5. Lemon-juice?	104	12	90	10
6. Quinine?	66	40	62	38
II. Think of some person who is well known to you, but whom you have not seen for some little time.				
a, 1. Can you see the features distinctly?	87	31	74	26
2. The outline of the figure?	115	3	97.5	2.5
3. The colors of the clothes?	110	7	94	6
b, 1. Can you hear the person's voice?	86	31	72	28
2. Can you recognize your friends by their voices?	116	0	100	0
3. Can you call up the note of a musical instrument in its appropriate clang-tint: piano, harp, organ, bassoon, flute, trumpet?	106	11	91	9
4. Can you hear in your imagination a note that is too high for you to sing?	105	12	90	10
Think of the playing of an orchestra.				
5. Can you hear two different instruments playing together?	90	24	79	21
6. More than two?	51	55	48	52
7. Do the tones ring out in their natural loudness?	49	54	48	52
8. Do they come to you from their natural places in the orchestra?	70	27	72	28
c. Can you hear in memory				
1. The beat of rain against the window panes?	113	4	96.6	3.4
2. The crack of a whip?	105	10	91	9
3. A church bell?	108	6	94.7	5.3
4. The hum of bees?	100	15	87	13
5. The clinking of teaspoons in their saucers?	103	11	90	10
6. The slam of a door?	114	2	98.3	1.7
d, 1. Can you see the person in familiar surroundings?	116	2	98.3	1.7

2. Can you see more of these surroundings (*e. g.*, a room) than could be taken in by a single glance of the eyes?	75	26	74	26
3. Can you mentally see more than three faces of a die, or more than one hemisphere of a globe at the same instant of time?	47	70	40	60
e, 1. Do you possess accurate mental pictures of places that you have visited?	113	4	96.6	3.4
2. Do you see the scenes and incidents described in novels and books of travel?	102	14	88	12
f, 1. Are numerals, dates, particular words or phrases, invariably associated in your mind with peculiar mental imagery (diagrams, colors)?	64	54	55	45
2. Are certain sounds always connected with certain colors?	37	81	31	69
3. Have you any other constant associations from different sense-departments?	35	76	32	68
4. Have you a special gift or liking for mental arithmetic or mechanics?	21	93	18	82
5. Can you lay a plane through a cube in such a way that the exposed surface shall be a regular hexagon?	13	103	11	89
6. Through an octahedron?	10	105	8	92
7. Have you ever played chess blindfold?[1]	0	104	0	100
Explain fully how far your procedure in these cases depends on the use of visual images.				
III. Think of the national anthem.				
a. 1. Can you see the words printed?	90	28	77	23
2. Can you hear yourself say or sing them?	102	15	87	13

[1] One student says: "I have never played chess, but can play checkers blindfold. All the figures and the board are clearly defined in my mind."

3. Can you hear a company singing them?	100	7	93	7
4. Can you feel yourself forming the words in your throat and with your lips and tongue?	108	9	92	8
5. Can you hear the organ playing the air?	100	16	86	14
b. 1. Do you recall music easily?	65	52	56	44
2. Do you make up tunes in your head when you are thinking steadily or in reverie?	52	61	46	54
3. Does imagined music take any considerable part in your mental life: *i. e.*, do airs and motives and snatches of music play or sing themselves to you during the various occupations of the day?	81	29	74	26
4. Have you an 'absolute' memory for music: *i. e.*, can you identify a note that is struck upon a piano key-board, or tell the pitch of a creaking door?	14	103	12	88
c. Partly open your mouth, and think of words that contain labials or dentals: 'bubble,' 'toddle,' 'putty,' 'thumping.'				
1. Is the word image distinct?	31	86	26	74
2. Can you think of a number of soldiers marching, without there being any sympathetic movement or movement-feel in your own legs?	36	81	31	69
3. Think of getting up from your seat to close the door; can you feel all the movements?	103	14	88	12
4. As intensively as if they were really made?	32	67	32	68
d. 1. Are you stirred or moved as you think of the words or music of the anthem?	70	43	62	38

2. Are you affected in this way at the theater, or when reading novels?	109	9	92	8
3. Do you choke and cry (or feel like crying) as you read, *e. g.*, of Colonel Newcomes' death?	90	14	87	13
4. When you think of your childish terrors, or of your childhood's injustices, do you feel over again the fear and resentment?	71	43	62	38
e. If you see an accident, the crushing of a limb or the catching of a finger in the door				
1. Do you yourself feel the blow and bruise?	61	49	55	45
2. Does the sight make you shiver, give you 'goose-flesh?'	99	13	88	12
3. Do you pant or hold your breath as you watch a difficult feat of climbing or trapeze-work?	99	14	88	12
4. Can you in general call up organic sensations; hunger, thirst, fatigue, feverishness, drowsiness, the stuffiness of a bad cold?	80	36	69	31

IV. Arrange the following twenty experiences in groups according to the clearness, vividness and distinctness with which you can remember or imagine them.

a. A gloomy, cloudy sky; a sheet of yellow paper; a black circle on a white ground.

b. The feel of velvet; of dough; of a crisp dead leaf.

c. The smell of tar; of a fur coat; of an oil lamp just blown out.

d. The taste of chocolate; of olives; of pastry.

e. The warmth of a hot water bag at your feet; the cold of a piercing wind that cuts through your clothing.

f. Singing in the ear; the buzz of an induction coil vibrator; the preliminary a′ of the violin.

g. Nausea; toothache; pins and needles.

V. Give any supplementary information that occurs to you on the topics of this questionary.

1. Do you recollect what your powers of visualizing, etc., were in childhood?				
2. Have they varied much within your recollection?	46	32	59	41
3. What difference do you find between a very vivid mental picture called up in the dark and a real scene?				
4. Have you ever mistaken a mental image for a reality when in health and wide awake?	23	82	22	78
5. Are the characteristics of your mental imagery repeated in the other members of your family?	13	8	62	38
6. Have you a good command of your images?	73	9	89	11

VISUAL.

All answer the first question in the affirmative. Two partial exceptions, however, should be noted. One says the colors are distinct, but not natural, the other that she cannot image the green in a bunch of flowers. In neither of these cases is there any lack of a general ability to image colors as is shown by their answers to other questions. While in answering No. IV. only a few give the image of the yellow paper unmistakably the first place, no one places it among those that cannot be imagined at all. Two other questions involve color imagery: the drop of blood on the finger [I., *g*, 2], and the colors of the well-known person's clothes [II., *a*, 3]. Only four fail to recall the drop of blood, and seven the colors of the clothes.

One of these four is among the seven, but as she sees the color of the flowers and the yellow paper, the negative answers in these two cases must be due to special circumstances, and do not indicate an inability to form color images. In spite then of a few negative answers to special questions, we find no one of the 118 lacking in images of color.[1]

As to illumination there is no such unanimity. While all but twelve see the box of flowers in a good light, nearly half (52 out of 111 who answer this question) say that the image does not seem as bright as the real object.

The imagination of definite forms is as general as that of color. Only three do not have the flowers, leaves, or box, well defined and clear cut. And three again, but not the same three, do not see the outlines of the friend's figure. All see the black circle on a white ground.[2] While, as just mentioned, only three fail to recall the outlines of the friend's figure, 31 cannot recall the features. A considerable number find that the features of intimate friends (parents, brothers, sisters, etc.), cannot be recalled as clearly as those of mere acquaintances or persons seen only a few times. One student expresses a great feeling of relief at finding that this inability to recall her parent's features, which had often troubled her, did not imply any lack of affectionate sensibilities on her part. Forty-three per cent. do not see the entire group of objects together with equal distinctness (I., *c*, 2). Galton reports exactly the same percentage of Charter House boys who have the field of view decidedly contracted. The question as to the ability to see more than three faces of a die or one-half a globe at once is answered in the negative by 60 per cent. Armstrong reports 69 per cent. of the answers to the same question as negative or doubtful.

Accurate mental pictures of places visited are almost universal. Four only answer this question in the negative. Fourteen, however, do not see the scenes and incidents described in novels. This seems to indicate a somewhat lower percentage

[1] Armstrong (PSYCH. REV., 1894, p. 508) reports 9 per cent. of young men students who do not have color imagery distinct and natural.

[2] There seems to be no reason in these cases to justify the statement of Galton (*Mind*, 1880, p. 313) that color is more easily recalled than form.

of imagination images than of memory images. Of the 90 who see the printed words of the National Anthem, a considerable number recall only the first few lines or even the first few words only, while 28 do not see the words at all. Few seem to have vivid visual images of printed words, while many retain the general look of the page and of the part of the page on which anything occurs. Two or three, however, say that they find it necessary to fix the visual images of the words in mind in order to learn anything verbatim. One visualizes words so vividly that she can spell backwards as easily as forwards.

Sixty-four report some sort of mental images habitually associated with numerals, dates or particular words and phrases. The greater number of these associated images are of a very simple character—a column of printed figures with numerals; some picture in a history book with a date. For example, quite a number say that 1492 always brings up a picture of Columbus landing. The following are some of the associated images mentioned: Date 1066 calls up shape of southern part of England; even numbers blacker than odd ones; certain names associated with color; the centuries located in different spots on the map of Europe; word 'fomentation' suggests cream-white; dates associated with colors; months are colored; names of persons and months associated with colors; peculiar words associated with circumstances of first hearing; numerals from 1 to 9 seen as they appear on dominoes, other numerals seen as figures. Various symbolic diagrams for the arrangement of the months in the year and the days in the week are mentioned, most of which seem to have been derived from calendars. As to the variation in the power of visualizing since childhood, the answers are for the most part very hesitating. Thirty-two think there has been no marked change within their recollection, 25 think there has been a development or increase in their ability to visualize, and 21 think that on the whole there has been a decrease in this capacity. Several report an increase in the power to visualize since entering college, due to the training given by the department of English incident to the work in composition writing.

The differences between a vivid mental picture called up in the dark and a real scene are found to be quite imperceptible by 21, five even find that the mental picture is more vivid, stronger, or in some way superior to the real perception. Fifty-five report the image in some respect less perfect than the perception. There is a large variety of ways mentioned in which the picture differs from the real scene. One says the image fades and brightens rhythmically. Several say that the mental picture is smaller than the real scene. Miss F. writes: "A vivid mental picture called up in the dark and a real scene are identical as far as the scene proper is concerned; but in the former case, the scene is seen thrown into an infinite black frame, frame *not* background." Other statements are as follows: In a mental picture each object is called up separately, in a real scene the general idea of the whole is gotten at once; the outline of the image is sharper than in the real scene; the image seems darker than in the real; the image is the same as the reality except as it requires effort to see it; the image is smaller and the details less vivid; the mental picture lacks color and life; as vivid as the real scene but seems infinitely smaller; image has no sound; image seems unreal and off in the distance; mental picture called up in the dark appears as in black and white; figures smaller, puppet-like; image has only the things of interest in a clear light; people in mental picture do not move and seldom speak; the difference is in a kind of unnatural chiaroscuro; the mental image never lasts long, and having a tendency to change requires a conscious effort to keep it before the mind; no distinguishing marks, but does not seem real; all the objects seem darker although they are just as distinct. We may conclude fairly that all of the class are good visualizers, but there are some whose mental images of other kinds are more vivid. Of 84 replies to No. IV. which indicate the precedence of one class over the others, 20 give an image of some other class than the visual the first place.

AUDITORY.

The tests of auditory imagery in the questionary are very complete. Pretty much every phase of sound is represented—tones, noises, voices, music, timbre, pitch. It is interesting to

note that while there are none who cannot recognize a friend by the voice, 31 out of 117 cannot recall the voice of the person imaged. Certainly recognition does not depend upon the revival of memory images, as some psychologists seem to teach. Some report that they cannot hear the note too high for them to sing unless they imagine some one else singing it. This indicates an inability to separate the sound proper and the motor sensations in imagining their own voices. A similar close connection between motor and auditory vocal images seems to be implied in the case of one student, who says she can identify the notes of the piano, but only within the compass of her own voice. That only 15 report that they are unable to hear themselves say or sing the words of the national anthem arouses the suspicion that some may have mistaken the motor images of the words for the true auditory.[1] But this suspicion is perhaps unwarranted, for there are only 12 who fail to recall the note too high to sing, and all can recall at least some of the noises. Two of the 15 are among the 12, and 9 of the 15 are among the 52 who do not recall music easily. Comparing all the answers to the various tests of sound images, I find that no one in the class is lacking in the images of either noises or tones. In arranging images according to their vividness, 3 put auditory images in the first place and 12 in the second place. But for the specially difficult character of the examples suggested (IV., *f*) their number would, I think, have been much larger.

[1] My own inability to experience vocal imagery of any but the motor type has perhaps made me too suspicious on this point. Baldwin says, "I cannot recall the words of a song until I get the tune" ('Mental Development,' p. 442), and again, "Internal tune is almost entirely auditory" (p. 439). My own introspection finds exactly the reverse. Internal tune is almost (if not quite) entirely motor, and I cannot recall the tune until I get the words or the motor sensation of at least inarticulate humming. I have never been able to discover any distinct and separate auditory images. If I have any auditory imagery at all it is as a shadow accompanying the more substantive motor images. That the motor images of my voice have this auditory tint or shadow seems borne out by the fact that imagined music excites in me in a faint way the same emotional effect as music actually heard. I feel, therefore, that I must plead an exception to the rule announced by Lay (PSYCHOLOGICAL REVIEW, Monograph Supplement 7, 1898) that nobody except the born blind and deaf would deny the existence of visual and auditory images. My hearing is ordinarily good, but as far as my own experience goes I should deny the existence of auditory imagery. Of course I do not doubt the existence of auditory images in others.

Nearly one-third report constant associations of sounds with colors. There is one quite numerous group of these associations which show a close similarity. Examples of this group are the following: The higher the sound the lighter the color, and *vice versa;* loud sounds dark, high sharp sounds light; low sounds dark, high white or yellow; high harsh sounds associated with bright gaudy colors like red or yellow, softer gentler ones with lavender and blue; high shrill sounds with cold colors, gray and blue, lower fuller ones with red, rose and bright warm colors; thin rasping sounds suggest lighter tints, blues and greens, deeper fuller tones suggest reds; high notes always bright hard blue color; high notes associated with the light colors and brightly lighted objects, low sounds with soft lights and dull or dark colors. A few of the more or less miscellaneous associations of sounds and colors may be mentioned. A thin high voice, white; deep voice, green; thunder, black or gray; beautiful music calls up beautiful scenes; sounds of violin bring up blue, of drum red; a harp sounds yellow; a trumpet blast red, note of piano blue; the sound of a certain voice is white; certain laughs always seem purple.

Tactile.

One member of the class answers the questions as to feeling the softness of the rose petals, the roughness of the ferns, the stiffness of the box and the prick of the thorn all in the negative. She remarks that she gets 'the textural effect by sight alone.' She does, however, recall very faintly the feeling of velvet, dough and a crisp dead leaf. This is the case that comes nearest to having no tactile imagery. Four find their tactile images the most vivid of any, and 23 put images of this class in the second place. Some mention associations of tactile and color imagery, *e. g.*, everything soft feels gray, many harsh goods yellow; colors feel differently to the hand, red feels rough.

Gustatory.

Two only answer all the tests of taste in the negative. One of these remarks that she cannot recall taste but has accompany-

ing muscular sensations.[1] Two again put the taste images first in order of vividness and seven put them second. One tells of 'the immense amount of pleasure and satisfaction' she received from eating imaginary ginger-cookies at a time when her appetite had returned after she had had typhoid fever and when she was not yet allowed to take anything but broth. Another says she can call up scarcely any recollections of taste and smell except unpleasant ones, 'for I have a very heavy cold.' Associations of taste and color are mentioned; *e. g.*, one says tastes usually bring up certain colors; another, onions taste green; certain tastes are pink, others green, and one flavor is brown. One tried with success the experiment of tasting in memory one food while eating another. "I found," she says "that I could taste things with a strong flavor (the best results were obtained by a salad dressing of olive oil and vinegar in which the vinegar was very strong), but that while so doing I could not taste the food I was in reality eating."

Olfactory.

Two are unable to imagine any of the examples of odor suggested, and one of these two is also one of those who have no taste images. Four put an image of smell in the first rank in order of vividness, eight in the second place.[2]

Thermal.

One student answers all the tests of heat and cold images in the negative. Two, on the other hand, put temperature images in the first rank, and 10 in the second rank. There are some associations of temperature with color mentioned. One says certain colors are always associated with temperature; another that red suggests a loud noise and heat, green feels cool, another too associates red with heat.

[1] This is exactly my own experience.

[2] Cf. Wundt ('Human and Animal Psychology,' p. 286): "It is in most cases illusion when you think you can recall the scent of a rose."

Bentley (*American Journal of Psychology*, October, 1899), "Images of taste and smell are comparatively rare."

On the other hand Dr. Gamble says fully half of her 65 Wellesley subjects had smell images (Titchener's Experimental Psychology, Instructor's Manual, p. 393).

Motor.

There are nine who report that they do not feel the words of the anthem in the throat or with the lips or tongue. Two of these nine can think of the labials and dentals distinctly with the mouth open, and three including these two do not have the sympathetic movements when thinking of soldiers marching, and also belong to the 14 who cannot feel the movements of getting up and closing the door. Several report that they have the feeling of movements in sympathy with the marching soldiers only if they imagine military music at the same time. As far as we can judge from the few tests offered, there are two or three who lack motor imagery. Among the twenty experiences to be arranged according to clearness and vividness there are no motor images suggested.

Pain.

Fifty-two do not feel the smart and soreness from the imagined prick of a thorn. Seven cannot recall the toothache, three of whom are among those who do not feel the pain of a thorn prick. Closely allied to the pain images are the sympathetic feelings excited by witnessing an injury done to another (see III., *e*). All but 13 report some sort of feelings of this kind. Some say, however, that instead of feeling the blow and bruise, shivering and having 'goose flesh,' they feel faint or sick. One reports that in her mind pain has shape—*e. g.*, stomach-ache is hexagonal.

Organic.

Eighty report that they can call up organic sensations in general, though many of these are unable to recall some of those suggested. Several note that they cannot recall any image of organic sensations unless at the time they are inclined to have that sensation.

Emotions.

Seventy-one out of 114 report that they can feel over again fear or resentment experienced in childhood. One of two who do not recall fear or resentment, recall embarrassment. That so large a percentage can recall childhood's emotions, goes far to justify those who maintain that affective states can be revived. Still

this does not by any means settle the question. The distinction between a revived resentment and a present sympathetic feeling of resentment at the cruel treatment of the little child imagined in mind, must be slight at best, and it seems to me quite possible that even as many as 71 observers may have mistaken the latter for the former. Very few answered the question as to the similarity of their own imagery and that of the other members of their family, and those who did answer it expressed so much uncertainty that little weight can be given to their replies. As far as they go, however, they indicate some tendency on the part of mental imagery to repeat itself in the several members of the same family.

Hallucinations were reported by 22 per cent. Most of these are extremely simple, *e. g.*, one thinks she hears her name spoken and turns to find no one else in the room; another sees a handkerchief on the floor, stoops to pick it up and finds nothing there. There are several fairly good ghost stories told, however, but these are all isolated experiences except one. This case is described in the words of the subject as follows: "I have one mental image that returns to me constantly, and has for more than a year. If I fall asleep on my left side, I awake almost instantly and see some one standing by my bed. The figure is always the same, and I see it until I close my eyes or get up and light the gas. No one else in the family has ever had such a mental image."

Considering the papers as a whole, I should say that the differences in mental imagery of the several members of the class are almost entirely a matter of degree. All are able to call up visual, auditory and tactile images. Only one or two in each case are lacking in either taste, smell, temperature or motor images.[1] This almost universal capacity for all kinds of sense imagery is due, I think, to two causes. First, the subjects are young women. Just how far this cause is operative can be told only when an equal number of young men are tried by the

[1] Stetson (PSYCHOLOGICAL REVIEW, 1896, p. 403) says of 100 Oberlin students, "None lacked visual or auditory images though one considered the auditory doubtful. One lacked motor images, three lacked tactual images, some four were without much imagery."

same set of questions. Second, there is a relatively large number of tests offered for each class of images. Those who could not recall one of the examples suggested, could some of the others. If one were to generalize from this single set of answers, he would conclude that in most people the mind is capable by effort of all kinds of sense imagery, although as a usual thing its content is limited to one or two special forms.

DISCUSSION AND REPORTS.

NOTES ON SOCIAL PSYCHOLOGY AND OTHER THINGS.[1]

I. The Socionomic and the Social.

The criticisms of this book have made it plain—what usually occurs, indeed, when a large problem is approached from a restricted point of view—that further explanations are needed as to the presuppositions of the text. Dr. Bosanquet criticises it from the point of view of the philosophy of the state (or of society). But philosophy of society is one of the 'other things'—it is not social psychology. Dr. Bosanquet's complaint is that invention and imitation are not two things, factors, 'moments,' philosophically considered, but that they are only aspects of a single principle, 'identity in change'; and Mr. Ball follows Dr. Bosanquet. Philosophically this may be true. I, indeed, find Dr. Bosanquet's own views, in his work 'Philosophical Theory of the State,' in the main satisfying. I had myself indicated that my views might go very well with an idealism in social philosophy of the type held by Hegelians; but as a worker in science, in genetic science, where facts, oppositions, dualisms, and pluralisms of all sorts, are the material, his formula is the purest tautology. 'What doth it profit' the sociologist, the statistican, the reformer, the observer of this invention—say, the cotton-gin which transforms a great branch of industry—or of that imitation—say, a lynching party following a leader—'to gain the whole world'—the 'general will' which both may illustrate as identity in difference—and lose the soul—the concrete

[1] These notes are matter added in the forthcoming third edition of the writer's work 'Social and Ethical Interpretations.' Their form and certain allusions are explained by that fact. The following criticisms of the book are those referred to in what follows.

Criticisms of Earlier Editions of this Work.

J. Dewey, *The Philosophical Review*, July, 1898; and *The New World*, September, 1898. J. H. Tufts, The Psychological Review, May, 1898. H. Havard, *Revue de Métaph. et de Morale*, Jan., 1899. F. H. Giddings, *Science*, Jan. 6, 1899; and in 'Democracy and Empire.' S. Ball, *Mind*, April, 1901. W. Caldwell, *American Journal of Sociology*, Sept., 1899. C. A. Ellwood, *American Journal of Sociology*, May, 1901. B. Bosanquet, *Mind*, May, 1901, and in 'Philosophical Theory of the State.' P. Barth, 'Einleitung' to the German translation of this work (Leipzig, Barth, 1900).

social somewhat which distinguishes the two cases! Go to the biologist in the analogous case and speak thusly: "Cease correlating and measuring variations, and cease figuring out hereditary likenesses: the principle of life is the principle of identity in change." He will reply: "Indeed, quite possibly." But his work will go on, and he may say further: "It was not on your formula that the modern evolution theory was established, nor by it did Darwin discover natural selection; but the rather by observing variations as such, and cases of its opposed principle, hereditary resemblance." I have said as much in the section on Hegel's views[1]: what is wanting, to bring science to the support of philosophy, is a formulation of the actual uniformities and oppositions, and the discovery of the processes by which these occur. This is the business of social psychology, on the one side, and of the social sciences on the other. Social philosophy is 'another thing.'

Then there is biology, and with it individual psychology, as such. These, too, are 'other things.' It is interesting to find the distinction made in Sect. 313*a*—between social forces as such, and the limiting and directing conditions under which they work—and covered under the terms 'socionomic' and 'social,' recognized by Comte (cf. Barth, *Philosophie der Geschichte als Sociologie*, I., p. 33 f.). The neglect of it since Comte is remarkable. My critic, Professor Ellwood, fails to observe it, and so charges me with neglect of these 'other things.' I am not second to anybody in the recognition of the biological forces —of natural, artificial and sexual selection, of struggle for existence, of competition of types and of group selection—as conditioning and directing social evolution. But my work is to investigate the *social*, not the socionomic: the forces implicit in the social movement—the uniformities, oppositions, and processes of social change. These are always inside the social groups, not between social groups; if between them, then by this very fact they become parts of a larger group within whose movement the social forces are immanent. These 'other things' belong to the sociologist, who aims to discover all the conditions as well as all the properly social forces of social history; but not to the social psychologist. And even then it is his business to recognize fully in the spirit of Comte's distinction—what he never does!—these

[1] It may be noted that I have gone so far, in a footnote, as to say that the philosophical supposition of a 'real or general self'—that is a truer way of speaking, than of a 'general will'; truer to the facts I mean—implicit in the whole process of social organization is at least not excluded by my 'self-thought' theory. *Qua* philosopher, one might say more!—but only in a philosophical context.

socionomic forces as outside the truly social. The biologist often falls into the same confusion, calling the geographical environment and natural selection biological forces; but as soon as we substitute 'vital' for 'biological,' we see his error. Professor Ellwood's criticism on this point, therefore, not only fails to reach home; it illustrates what is to my mind a common and glaring confusion of thought (unless, indeed, it is in the interest of general sociology that he writes; for in that case, apart from details, I, *qua* sociologist, accept most that he says).[1] It is just this sort of confusion of things and 'other things' that makes this whole subject the pseudo-science that it is to-day in the eyes of many.

The same—to come closer home—is to be said as to the relation of individual psychology to social psychology. Only these mental states and processes which are 'social' as now defined belong to social psychology; only those which are, actually *are* elements—not merely condition, limit, advance, hinder states that are elements—in a whole which implicates more than the strictly private life of the one person that has them—only these! A distinction is marked by the terms 'autonomic' or private, and 'socionomic' (public, social) in my note in Groos' *Play of Man* (Eng. trans.), p. 4. Groos divides the "impulses by which the individual wins supremacy over his own organism without regard to other individuals * * *" from "such other impulses as are directly concerned with his relations to others." Not inquiring whether what is strictly private or autonomic actually exists, we may say that a large part of the individual mental life is at least socionomic (just as we say above that the biological very often is); but now we ask further: what part of this is truly social in the narrow sense of being intrinsic and essential to a social, and to every social, situation? This alone concerns us for our problem, although one might define social psychology more widely as including the socionomic in general. So much said—Professor Ellwood brings the charge that I do not allow for various of these socionomic mental processes by which the social life is conditioned and its direction determined (as, for example, the impulses of rivalry, acquisition, sex, the emotion of fear, etc.). Of these, I have to say that they are real and powerful things, and to them the social life often owes its direction, its variations of character, its forms of operation, and much beside. A writer on sociology must be true to psychology on all these things, and much of my book, as

[1] The same applies to the criticism of Professor Giddings, which I find, however, difficult to deal with on account of the crude and ill-considered psychology. See below.

Professor Ellwood truly says, is devoted to them (Part III., 'The Person's Equipment'). Why then—he goes on to ask—is the psychological factor in social organization limited to one impulse, 'imitation,' and to one form of mental content, 'thought'? The answer is that the doctrine does not disregard the others—the 'other things'—of psychology; but it finds them socionomic only, not intrinsically social. Sometimes they are there in a social change, many or few of them; sometimes they are not; what is always there, the psychological thing which is essential and sufficient, is the sort of thought which I call 'self thought,' and the mode of its growth and propagation, imitation. Given these, social life is possible; but there still remains the relative determination of it by the 'other things,' to be worked out by the sociologist. This is my view; and such is the reason that the true and powerful factors which my critic dwells upon are not made more of in a work on Social Psychology.[1]

To say, as Professor Ellwood does, that such a process could go on in a vacuum is—respectfully submitted!—nonsense. The banks are not the river, but where is the river-course without banks? Chemical processes are not of themselves vital, but where is life without H_2O? Similarly where organic evolution without the bionomic?—and this quite apart from the theory of imitation which Professor Ellwood is mainly criticising (and which, I may say, is not mine).

So here, as elsewhere, there is a gradation, a hierarchy, in science: chemistry necessary to life, but not itself of life; forces in the environment necessary to evolution, but not themselves vital; life processes necessary to consciousness, but not themselves mental; consciousness necessary to society, but not all consciousness social; social consciousness necessary to social organization, but not all social consciousness actually in a social organization. In every department of science there is much that is 'nomic' (conditioning, determining, directing), but not intrinsic to it. Whether or not this be accepted as a working distinction in this case, it will nevertheless serve to show what the chapters 'Social Forces,' 'Social Matter and Process,' and 'Social Progress' in this work aim to accomplish, and also to take point from the criticisms that they do not recognize those 'other things' which are really outside their scope and aim.

A further word in this introduction on the general class of topics covered by the term 'sociology' may not be found irrelevant to the

[1] The criticism (by Ellwood and Giddings) that I here make a break in the evolution process is, I think, entirely without force, as I aim to show in a later connection (see below).

main subject-matter of the work. In my view the special social sciences furnish foundations for a general social science, and this latter is sociology. It deals (1) with the nature of the social as such: what is common to the special social sciences—its analytic branch—and (2) with the natural history of societies: the problem of social evolution—its genetic branch. The reader may consult the table given in my *Dictionary of Philosophy and Psychology*, art. 'Social Sciences,' showing the logical and methodological divisions of such a general science. The same state of things appears in biology (a very similar table is given in the same work, art. 'Biological Science,' with the added authority of Professor E. B. Poulton), and the same distinction between 'general' and 'special' is in common use in that department of science.

II. Animal Companies and Human Societies.

It remains to gather up the facts regarding the forms of quasi-social organization among animals. The distinction made between human and animal common life is, of course, not a hard and fast one. The types are respectively types, not kinds. In saying that man is a being whose social life is an organization arising from his growth as a self—as a being who thinks himself and so thinks others also in relation to self—is not to say that there are no factors in his social life due to the lower functions—impulses, emotions, instincts, etc. Man is also an animal. He has certain spontaneous tendencies companywards, apart from his great capacity to think himself into conscious social life. This, however, if it were all he had, would lead to the sort of gregarious life called above 'socionomic'; that is, in the main. That is what the animals have. In its *type* it is a life together, because it is natural for them to live together. It represents the 'instinctive' and 'spontaneous' periods of equipment. This fully admitted—that there is such company life among animals—we yet find it different from the human, just as the child's early spontaneous reactions—bashfulness, organic sympathy, etc.—are different from his later reasonable and reflective attitudes. Yet the transition is gradual, as the springing up of the form of organization called the 'self-thought' situation is gradual. I have endeavored to show the child's progress in actually passing from the lower stages into the higher. So with the animal forms: they are *mainly* instinctive, *somewhat* spontaneous, *a little* reasonable—in the highest species,—*never* ethical. If the individuals of a particular group have a germ of self forming within them, then their organization is becoming tinged with true 'social' value, though in its

type it remains still that of a 'company.' The criticism (Ellwood) that I find here a break in the genetic line—an impassible gulf between animals and man—is contradicted by my whole view of the social life as a gradually developed thing emerging with the consciousness of self. Yet, this continuity of development assumed, the point emphasized in the foregoing pages is the fact of a growing and typical difference between that gregarious consciousness which mainly reflects fixed and unprogressive nervous functions biologically selected, and that consciousness which, becoming freed from these limitations, shows its capacity for the psychological organization which is intellectual and ethical. To this latter alone I apply the term 'society'; to the former, 'company.'[1]

III. The Process of Social Organization: Imitation.

The discussions so far assume a certain definition of imitation, and also a distinction between the function itself and its exhibition in social life. By definition, I understand imitation to be either (1) a process in which one individual uses another as a copy for his own production of something, whether or not he intentionally and consciously aims at the other as his model; or (2) the same type of function when that which is imitated belongs to the imitator himself instead of to another person. The first of these phenomena I propose to call 'social imitation': it is the sort of imitation described mainly by the sociologists (Bagehot and Tarde). Psychologically, the latter is that which is called the 'imitative function,' or 'psychic imitation,' as mainly dealt with by the psychologists (Royce; the present writer, in 'Mental Development').[2] As type of function, this cannot be denied the name imitation, for the process of imitating a copy is precisely the same in the imita-

[1] Criticisms of the view that the social matter is the 'self-thought,' turn largely on the necessity of recognizing the animals' gregarious activities. This we may fully do; but the problem then still remains: how can we get human society with its characteristics?—*i. e.*, (1) reflective opposition to or confirmation of the gregarious impulses; (2) the universality and publicity of social duties and rights; (3) the peculiar 'general' or will self; (4) the institutions in which all these are embodied, notably the state. These things are so outstanding!—'man with his social history is so different from the brute with his physical heredity!'—that the real need is to bring out the human factors, not to obscure them. See, for example, the inadequate outcome of the biological (as opposed to a psychological) naturalism of such a writer as Sutherland (*Origin and Growth of the Moral Instinct*).

[2] The purely neurological self-repeating function which in that work I called 'organic imitation,' is better known as 'circular' process. It underlies, however, in my opinion, all the higher imitative functions.

tor's consciousness, whether the copy arise in his own mind, or be introduced there by another person. But the social phenomenon is social simply and only because there are two or more persons necessary to the imitation, and hence the confusion arising from the failure to discriminate the two points of view. Psychological writers have been careful to mark off the sphere of 'self-imitation' (by this term) from that of 'social imitation.'[1]

In the study of social process, it is clear, we may take the point of view of social psychology—that of the question: by what mental process men actually are social and show social organization? But it is possible also to take the point of view of sociology—that of the question: what do I as an observer find going on between or among men who are socially organized? If one replies to the first question, 'imitation,' he means a different thing from his possible reply, 'imitation,' to the second question. By saying that the social process is imitation, I mean, for example, more than M. Tarde does, who speaks from the objective point of view. In short, the observer sees often what is not 'social imitation' going on about him; he sees opposition, invention, discussion, etc.; and often he sees in imitation less—he sees social imitations which are not productive at all for social organization. But from the point of view of social psychology—of psychic imitation as a function of the individual's life and growth—all of it may still be imitation of this latter type. This is what I believe; it is, indeed, implicit in the foregoing pages, and is now to be brought out more plainly.

We may approach the subject from the point of view of sociology, and ask for the limitations of the sociological theory of 'imitation.' These have been brought out by many recent critics.

First, we are told that much imitation of one by another is not fruitful. This is true (see the criticism of M. Tarde's view, section

[1] In the careful treatment of the terms 'Imitation,' 'Copy,' 'Model,' etc., in my *Dict. of Philosophy*, the topic is brought into line with others cognate to it under the headings 'Mimetism' and 'Resemblance,' where the term 'mimetic resemblance'—that in which what *is resembled* is itself a factor in the production of that *which resembles it*—is made to cover both cases of imitation. The use of the term 'instinct' as applied to imitation is, I think, confusing. As used in my *Mental Development*, it means simply what is a native tendency or impulse, not an instinct in the sense of a function having a fixed form of reaction or expression. I now follow the recommendation of the *Dict. of Philos.*, and call it the imitative impulse, which, I think, is native for the reasons I give in arguing the case in *Mental Development*. Cf. Groos's similar revised usage in this case, and also in that of the play impulse (*Play of Man*, p. 2).

316, 1), but it may still be true that what is fruitful always involves psychic imitation (or even social imitation). This criticism holds only against the view that social imitation is always fruitful for social organization, which I think is far from correct.

Second, we are told that although imitation may be present, it is not that which is fruitful and essential: (1) the recognition of another self, (2) the constraint of obedience enforced by another, (3) the compulsion of ideas, (4) the onward sweep of the social current, (5) the sharing of a 'general will, (6) the recognition of duties and rights, (7) a social contract—all these are urged, and urged by those who criticise the 'imitation' theory.

Again, we may say, this negative criticism, coupled with most varied positive views, holds only against the theory that 'social imitation' is the essential and the only essential thing. But admitting its force, we ask: Are all the things mentioned as real social agencies—or any of them—adequate without psychic imitation, without the exercise of the imitative function in the social individual? And we find that they are not. They all involve a form of social matter which can only have arisen, and can only be operative, in a social situation through the imitative function. We may take them up in order:

(1) The recognition of another self, or of many other selves. This is only possible when and because the self-thought has grown up through direct social imitation with the further use of the same thought by ejection, which is self-imitation. The self arises through the reinstating, by imitation, of a copy found in others, together with the reading back of the enriched self-copy into the others. If the taking over from you to me is imitation, how does the function differ when I carry over from me to you? If this be true to psychology, then the recognition of another self is an imitative function through and through. At any rate this is a position which is not touched by the criticism in question aimed at the 'social imitation' theory.

(2) Constraint and obedience. Here the lesson taught, the task enforced, the obedience required, depends upon one's accepting and acting on what one is told; and acting on what one is told is a form of self-imitation which is only one step removed from direct social imitation. Where is the difference in my function between doing 'what I see you do and doing what I hear you tell me to do'? It is said the motive for the doing is different; and so it is. But it is the entire act which is or is not fruitful for social organization, not merely the motive to it. To be sure, the motive makes a difference; but the motive as such is not the criterion of its social availability. A

whipped dog obeys from fear, and so may a whipped man; but the man's act, motived by his fear, modifies or confirms his social status in his thought and in that of others; the dog's does not.

So I hold that social constraint, all that compels and enforces, in so far as it is social and not merely 'socionomic,' is so through its acceptance and assimilation; and this is then subject to the law of all social material that it be taken up by imitation in the social agent's personal self-thought.[1]

So it is also with the factors mentioned above as (3) and (4)—the compulsion of ideas and the social current. These get in their work as strictly social only through their acceptance and assimilation by the social agent. The valuable data of M. Durkheim's book on *Suicide* can be fairly understood, I think, only on the supposition of a constant psychic imitation whereby the leviathan, society, finds his roarings echoed in numberless cries, the voices of the individuals who are the organs of society—and this in spite of M. Durkheim's strenuous opposition to M. Tarde's imitation theory. Only a social agent can be compelled to be sociable, and only he can be a social agent who is socialized. It may be true that social conditions compel a certain number of suicides a year; but it is also true that each man himself commits suicide—otherwise it is not suicide, but murder. One may say that I am wrong in making this socialization proceed by the one process of self-growth through imitation. It may be. But still this theory is not touched by the criticism which merely points out that social imitation is absent in this case or that.

(5) Into the 'general will'—postulated by still others—in my opinion, psychic imitation enters. To partake of a general will—or a general mind, or a general anything, 'general' meaning in some sense 'collective'—one's private will, self, mind, consciousness, must implicate others in a collective outcome. What is the good of a general will if the individuals do not reflect it? It is just of its essence that they do. But this involves some mental content not only *common to their thought severally*, *but also thought by them as common*. This is what I mean by 'publicity'; and I hold that this arises in a common imitative situation. A will is not collective simply when *n* individuals agree in willing this or that. Each must will this or that as collective—as belonging to a public of individuals in the thought situation in which he finds implicated as he himself is. This implication

[1] A similar result appeared in the chapter on 'Sanctions' (Chap. X.), where we found that social sanctions to be effective have to be taken up and ratified by the individual as 'personal' sauctions.

of all in a common situation by the thought of each is, I think, possible only through the imitative development of the self-thought. This, I may say again, may not be true; but the assertion of a general will is entirely incompetent unless one can show how a general will is a psychological possibility and just what its genetic factors are. Rousseau and, more recently, Bosanquet make no effort to do this. The latter criticises imitation loosely, without seeing that this theory utilizes the imitative functions to derive the general will; in criticising me, he is hitting blows on the plate which this theory is placing over an exposed joint in his own harness. Yet in the main I indorse his criticism of the *sociological* imitation theory.

The 'status' theories, which (6) hold to a recognition of duties and rights as the essential thing, and the 'contract' theory, which (7) holds to a social contract, both point out something in the main true, but not analyzed in its lowest terms. How are duties and rights possible?—and how is this or that status possible?—how does man come to give his adhesion to the contract implicit in social organization? These questions I have endeavored to answer by depicting the process whereby the individual, in growing to be a person—by the dialectic of his personal growth—is at once also a social person with a status, and with duties and rights.

So also a social contract—in any sense in which it exists at all—is the individual's ratification of all that the status or social situation means. This, therefore, comes to supplement these partial theories. A genetic theory points out the origin of the developed social life with all its phases; and if my way of doing it be correct, psychic imitation is an essential mental process in it all.

Our result, therefore, made now more clear from this review of criticisms, is that imitation is the method or process of social organization in two senses: (1) Ideas, inventions of all sorts, are actually propagated by the imitation of one man by another; but this is only one step in their conversion into social matter. Merely this fact of social imitation does not necessarily make these things socially available. If so, my parrot would, by imitating me, come into a social status with reference to me. Another factor is necessary, *i. e.* (2) imitative assimilation and growth, whereby what is imitated is organized in the individual's own thought, and imitatively ejected into others, becoming part of a situation—a status-scheme—whose organization includes 'publicity' and 'duties and rights.' It is only this full view, not the first part of it taken alone, that I am concerned to defend.

IV. Selective Thinking.

Mr. Bosanquet brings the positive criticism that I do not develop an exact view of the process of selective accommodation by which 'the mind can appropriate a law or principle, the scheme of a whole, and naturally and necessarily differentiate its reactions in accordance with the bearing of such a principle on the new situation presented' (*Mind*, 1899, p. 174). This is covered, I think, by the points given in the section on 'Selective Thinking' (Sect. 78), with the preceding sections on the nature of invention (Sects. 54–57), and carried out in detail in my later 'President's Address,' printed in the forthcoming volume, the third in this series, entitled 'Development and Evolution.' Briefly, I hold that in each such case the 'scheme of the whole' is itself the outcome of an earlier active accommodation (or of many of them); not only does action result in the selection of thoughts, but thoughts are the counterpart of former adapted actions. So in each case to recognize the ready-formed 'scheme of the whole' is merely to recognize the earlier organization—what in my 'President's Address' I call the 'platform'—by which the thinker is able in so far to 'size up' the new situation. The general process by which all accommodation is effected must go deeper than goes the assumption of a plan having itself no genesis; unless, indeed, we bring in intuition or some other form of 'preëstablished harmony' between thought and things. On my view the whole process is one of these phases: (1) the selection of actions which 'work' in a given situation, (2) the corresponding and consequent survival of the thoughts which are functions of such selected actions, and (3) the 'system of the whole' *so made up* which is brought to new situations; this last is but the mind's progress so far, in this line or that, in the two earlier mentioned phases of its growth. In short, the twofold psychological truth that (*a*) "what we do is a function of what we think," and (*b*) "what we shall think is a function of what we have done"—formulated in Sect. 57—cover the case, provided we admit that the 'functional selection' of movements from movement variations—constantly repeated from a progressive 'platform'—is the actual method of motor accommodation. In the case cited by Mr. Bosanquet (*loc. cit.*, p. 174)—the building of his new house—I should say that the plan of the whole is made up of parts each of which is taken imitatively from other houses or plans of houses, or selected out by the owner himself from alternative variations of thought, by the process of getting new workable combinations, which is indicated above. I could not wish a fairer example.

How, I may add, Mr. Bosanquet can hesitate, as he says, in re-

gard to possibly classing my humble self as an 'associationist' I cannot imagine. All my psychological publications have been from the first as diametrically opposed to associationism as an apperception theory, based on motor unity and synergy, can be. I am also of the opinion that Mr. Bosanquet will find in the later writings of Dr. Stout, from whom he takes the theory of 'relative suggestion,' evidence that that able writer is inclined to supplement his view, on the genetic side, by a theory of motor selection.

V. The Science of Society.[1]

To the Editor of Science: Your kind question as to whether I have any remarks to make on Professor Giddings' article on my book in *Science*, January 6, leads me to send the few sentences which follow. I should not otherwise have done so.

I have no essential alterations to make in my book on the topics Professor Giddings brings up. I find in Professor Giddings' 'consciousness of kind,' even more now than when I criticised it in my book, and for the reasons there given, the 'climax of descriptive vagueness,' seeing that I am conscious, in view of his successive statements, of a certain tendency to agree with Professor Small, that Professor Giddings uses 'consciousness of kind' as a sort of prospector's claim to anything which may hereafter be discovered. Indeed, Professor Small's review (*Amer. Journ. of Sociology*, January, 1899) of Professor Gidding's recent *Elements of Sociology* anticipates my reaction upon much of the latter's writing. When a third party informs one that one's preserves are poached upon, one does not mind saying one is oneself aware of it![2]

The truth is that Professor Giddings' way of treating psychological questions, together with the sources of the earlier opinions upon which he is now attempting to engraft the results of later psychological research, are so remote from my methods and sources that I fail to find, despite the best will in the world, much common ground for debate. For example, Professor Giddings says in the *Science* article that I just

[1] Written for *Science* (under date January 10, 1899), but not published. I disliked the personal, and judged Professor Gidding's review to be after all ephemeral. But he has now incorporated it in his work, 'Democracy and Empire,' and I change my mind and publish this.

[2] On the whole Professor Small's article accurately expresses my opinion, even more now (1901) than when this was written, of Professor Gidding's work, and I do not hesitate to express this conviction, since in the present state of sociological study, sober, patient, and accurate research—with the temper of it!—is the great desideratum.

missed making a 'really important contribution to social science,' and then, seemingly goes on to make it himself, for with his 'Elements of Sociology' there was actually issued a circular calling especial attention to his 'important developments of sociological theory,' and to 'a new contribution to psychology no less than sociology'—*on this very topic!*—or, at least, on this topic so far as I am able to judge. For although I find what Professor Small none too bluntly calls 'poaching' upon the preserves of Ward, Patten, and myself,[1] I cannot make out what the discovery is. Instead, I find, especially in the chapter of the 'Elements,' vague Spencerian analogies, and a show of novelty and finality imparted by dogmatic statement and the use of new terms.

Finally, I suggest that all of us, who think to do work in the borderland between two sciences, study to be informed each *in the other's Fach*, no less than in his own. I say this so fully aware of its homecoming thrust, that if Professor Giddings' colleagues confirm him in pricking some of my *sociological* bubbles, I shall let them collapse without a murmer; but the psychology—*das ist eine ganz andere Sache!* J. MARK BALDWIN.

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THE IMAGE AND THE IDEA.

Recent discussions in psychology and philosophy have revealed a fundamental source of difficulty in both disciplines. From the philosophic side the difficulty has been brought home by Mr. F. H. Bradley's very thorough treatment of the relation of 'content' to 'existence.' For psychology the question may be formulated in terms of the relation of images to ideas. It is with this latter aspect that we shall at present deal.

Since Galton's day the image has been carefully investigated and for our present purpose it will be sufficient to summarize the results reached. It is now generally agreed that although there may be an identity of function in the ideas of men, they nevertheless vary, structurally, with the individual, his time of life, his particular task. This distinction between the individuality, the warmth and coloring of

[1] To cite a case—besides those pointed out by Professor Small—in this immediate connection, Appendix D in my book may be referred to as putting in my way certain things that Professor Giddings puts in his way in the *Science* article. Even certain of my terms (as Professor Caldwell also notices), such as 'socius,' 'organic' and 'reflective' sympathy, are used with no intimation of their origin. In what he calls 'ejective interpretation and selection' I find, if I understand his meaning, partial statements of elements of my 'dialectic of personal and social growth,' yet stated—as is the case also with the other conceptions cited—in a form which I as a psychologist should not wish to have attributed to me.

structural processes and the colorless and prosaic character of determinate intellectual functions has been permanently recognized by the use of the terms *imagery* and *ideation*. Imagery is a name for concrete mental processes taken in their immediate and varied individualities. Distinct images are emphasized portions of this process, what James designates substantive states. Series of images constitute the imagery of separate moments as emphasized factors arise and pass away. These images vary in character and coloring to an indefinite degree, although typical forms predominate in each of us.

Ideas are not so easily described. To the elucidation of the nature of the idea and of its relation to the image the remainder of this essay must, therefore, be devoted.

Although the logical and metaphysical values of ideas have been exploited over and over again their psychology still requires much to be done. Even when we identify the idea with the concept we are safe in saying that the psychology of the process is still fragmentary.

As a point of departure the commonly received doctrine that *ideas* are *signs* or *symbols* may be accepted. As such they embody the *meaning* developed in the reflective activities of mind. But what are signs and how do they symbolize? Here the logicians come to our assistance. They have made clear to us that the meaning which universally constitutes signs consists in reference and in the relationships thus dynamically established. Every concept or idea is constituted by the sum-total of relationships embodied in its meaning. Granting this, the question arises, "How do relationships and references *exist?*" And at this point interest becomes acute, for over this question sensationalists and intellectualists have battled for ages. For the former, the reality of the idea has reduced itself to the rapid succession of a series of separate and more or less imperfect images. Relationship, meaning has either been done away with or reduced to a subordinate function of images. Every one is familiar with the challenge of Berkeley and Hume to show how else than in an image or a series of images the concept can exist. "Lay the finger," say they, "upon the concept and it melts away into images." The intellectualist, on the other hand, fearful of the consequences of sensationalism and persuaded of the inadequacy of separate images to explain the unity involved in meaning and relationship, has usually felt constrained to speak in a most mysterious way of the manner of the idea's existence. Instead of answering directly the psychological question of the sensationalist he has changed ground constantly and has expended his energies in proclaiming the necessity and value of related meaning. For this reason

the problem claims renewed attention. Contemporary psychology and logic have advanced the inquiry considerably. Logicians have pointed out that relations without terms, that reference without a base, are absurdities. Thus the intellectualist is forced to give a concrete answer to the question asked of him and to speak to the point. In this the psychologist is quite at one with the logician. On the other hand, the logician has conserved the true nature of meaning, reference, relationship in the presence of fleeting images. It has been demonstrated that before images can take on the function of reference, before they can become vehicles of relationship, they must be tranformed in a synthetic function which (metaphorically speaking) lifts them out of their immediate and individual natures. As we now view matters, the difficulty with both parties has consisted, first, in working from different points of view without knowing this, and second, in each setting up his own point of view as the only possible. The sensationalist has worked from an analytic standpoint and the intellectualist from a synthetic. For the former the organized whole—the idea—when dissected disappeared into separate terms or elements; for the latter no elements could constitute an organized whole or function apart from a synthetic act which gave vital unity to them. Thus we may admit that when we attempt to lay our fingers upon concepts they resolve themselves into flights of images, while at the same time we insist that there is a difference between an unmeaning succession of images and the idea of succession wherein meaning is realized, and that this latter involves a synthetic act by which the separable factors are organized in a related whole.

At this point the deliverances of psychology and logic become somewhat uncertain and we find it necessary to break new ground.

So far we may be agreed that ideas as symbols present a twofold aspect. On analysis they reveal the presence of images which constitute a base; taken as wholes, a synthetic function presents itself, a function whose dynamic nature is constituted by references or relationships. We must now make a more careful survey of these aspects.

And first it is to be noted that the relation between the base and the reference, the image and the relation, is inner and vital, not outer and mechanical. The two are separable factors and not separate elements. In other words, the psychical reality is a single activity, the symbol which for purposes of convenience may be viewed in two different ways. Image and reference being thus two different aspects of the same existence, we have answered one part of the sensationalist's question by agreeing with him in his contention. Ideas *exist* in

images. But when we press the question and ask how they exist, introspection makes apparent that images furnish simply the *base* of the idea, and the complete symbol involves primarily a synthetic function of reference into which the image is taken up and absorbed. Hence, in all processes of ideation imagery is constantly overlooked, and whenever meaning absorbs our attention the concrete characteristics retreat so far into the background as to fade altogether from view. We may illustrate the matter in this way: When we use a sign we never stop to consider the stuff of which it is made, but pass on to the objects which it indicates. Its meaning is, for the time being, our concern. But if for some reason we desire to examine the structure and make of the sign, we become aware, at once, of the stuff and its character. Stuff and reference, base and meaning, are (in fact) inseparable, although in normal use the stuff is subordinated absolutely to the reference. We know of no signs, no matter what their order, which exist apart from some sort of base. And yet mere stuff has no significance for us.

Having discovered that ideas are always based in images, we must next examine into the precise nature of the function which the image supports.

As already noted, the idea-function is constituted entirely of references, relationships, meaning—what James designates 'transitive' states. Now meaning or significance lies in determinate pointing—and this in two ways. First, the reference may involve merely the relation of one concept to another. Sign points to sign; meaning implicates further meaning. Second, the reference may involve not another abstract, but something concrete and immediate.

The first form of reference has to do with the interconnection of generalized meaning. The more determinate ideas become, the more general is their signification. Instead of being tied down to the limitations of particular times and places, the meaning is freed, abstracted. The more determinate scientific work becomes, the more completely it embodies itself in general, abstract formulæ. Furthermore, the human mind endeavors universally to organize these abstract meanings. Every idea as idea involves relationships beyond itself. And it is the part of systematic thinking to enlarge these relationships until each idea has been woven into a larger whole, constituted by the complex interrelationships of the component ideas. The ideal of abstract thought is to discover completely the relationships of each and every idea, *i. e.*, its real meaning. Granted then that the significance of ideas consists partly in the inter-reference of symbols, the

question arises once more, 'How is abstract meaning constituted in mind?' Again we are against the problem of the sensationalist and the intellectualist. And once again we must answer as before. It is impossible to believe, with the intellectualist, in what might be called a purely disembodied idea, one devoid of all connection with imagery. The most careful survey of mental states fails to reveal any such. The moment we analyze meaning, we find ourselves in the presence of imagery—visual, auditory, tactile, motor. On the other hand, imagery as such is not ideation. As reference is embodied in the connection of states, one with another, so meaning lies in the forecast and control of a series of states. It is quite true that every *meaning* involves the use of images, but the function of projection, its form and actual course, is a dynamic activity, which, if it cannot be separated from imagery, neither can it be resolved into mere imagery. If we are exact we shall realize that the meaning is the complete projected experience, consisting, from one point of view, of a series of imagery, from another of a dynamic transition from one image to another. In support of this general position several lines of argument may be advanced.

1. From a logical point of view the significance of ideas is divided into denotation and connotation. By denotation is understood the range of an idea's signification; by connotation the inner character of the signification. Psychologically the distinction is indicated by the use of the terms *general notion* and *abstract idea.* So far as the former is concerned, examination makes it apparent that the general notion is made up of a series of fleeting and more or less imperfect images. The general notion of triangle is represented (for the visualist) by a series of images of variously formed triangles or parts of triangles. Of these, the angles may be most pronounced, enlarging and expanding as the meaning may demand, while the sides may be faint and rapidly changing lines of connection. When we inquire into the connotation of 'triangle' a similar result is reached. A triangle is roughly described as a three-sided figure. Now this definition may be entirely verbal to us, or it may be real. In the latter case the presence and significance of imagery become apparent. In the case of the visualist, the appreciation of meaning involves the projection of three lines in such form as to enclose a space: and, so far as the connection of the abstract idea with the general notion is concerned, it is really *seen* that the variation of form does not detract from the use of the lines.

I have referred throughout to the representation of the visualist.

But although we must always reckon the differences of imagery in making up our account, such reckoning but serves to emphasize the general point which we have been making.

2. The second line of argument is founded in what at first hand appears to be a discrepancy. It would appear at times as though thought were constantly being carried on without reference to imagery. The more abstract our thought, the more habitual our lines of investigation, the more rapid the consideration of the moment, the farther removed imagery would appear to be. The source of the difficulty is not far to seek, however. It consists in the substitution of conventionalized for the more natural forms of imagery. The preeminent avenues of expression are the voice and the hands. It is no great wonder, therefore, that in our thinking, the imagery connected with these should in the average individual become predominant in thought. When, therefore, in abstract thought or in rapid reading ordinary sense images fade away, it is not that imagery has been abandoned altogether but that a new form has been substituted. Furthermore, the fact that in rapid reading or thinking imagery does not obtrude itself upon attention, is not to be wondered at. As has been remarked above, the *structure* of a symbol is always submerged in the function which it serves. Consequently, the more we think, the less we notice imagery. That in abstract thought imagery should be out of sight is precisely what we should expect.

3. Another proof of the close dependence of meaning upon imagery, is found in the constant resort to imagery when thought is baffled. So long as we use symbols which are quite familiar and so long as the combinations made from them fall within the beaten tracks of experience and habit, we pay little attention to the flights of images which bring home the significance of our thought. A mere suggestion is sufficient; our thought takes the required turn and we pass on. The moment, however, that some new thought or some new combination of thoughts arises, we search for the concrete imagery in which the conception may be appropriately embodied. Until we set the imagery before us, the thought remains vague, we are baffled. When the precise imagery flashes into mind a distinct sense of relief comes over us. The more vivid the picture, the greater is our assurance. Finally, if at any time we wish to fix a thought or to correct a mistake we turn almost involuntarily to the imagery and emphasize that. It is imagery which ensures a final *realizing* sense to ideation.

4. The study of aphasia furnishes additional grounds for a belief

in the inseparability of imagery and ideation. Destruction of centers upon which imagery depends is just as fatal to the existence of meaning as the destruction of the connections between centers upon which relational activities depend.

In the most abstract forms of thought, therefore, image and idea are inseparably connected. Meaning exists as a dynamic function of projection by which further experiences and thoughts are indicated. This projection, however, involves the use of imagery as a base, and, although the imagery is taken up and submerged in the function which it embodies, it is nevertheless present at every moment of thought. As we consider the next point this position will become more explicit.

In treating of the reference-function which constitutes the meaning of ideas it was remarked above that the reference might be of two forms: first, the reference of one symbol to another; second, the reference of the abstract symbol to something entirely concrete and immediate. Having examined the first form we shall now turn to the second.

Although reflection leads more and more to the determination and organization of abstract and universal symbols it still maintains its connection with the concrete and immediate aspects of experience. It is now commonly agreed that universal meaning cannot be separated ultimately from particular significations. Logicians emphasize the fact that universals do no more than set forth the identity of meaning observed in a variety of particulars. Scientists are fond of impressing upon us that laws have no meaning except as statements of the general methods of behavior discovered in facts. Psychologists also realize that no hard and fast line can be drawn between perception and conception. The distinction made between particular and universal, fact and law, percept and concept, is indeed not one of meaning, but one based upon the *use* to which the same meaning is put. Whenever meaning is limited to a single portion of space or time we have a concrete, a particular. When we abtract meaning from such limitation and use it in a free way to light up an indefinite number of singulars we have a universal. In other words every particular is a 'that-what' in which the 'that' stands for the discriminated images while the 'what' embodies the meaning, which, although it enlighten this image, may also enlighten an indefinite number of others. It is thus correct to say that ideas arise directly from immediate undiscriminated experience. The problem which confronts is, "What part does the idea ultimately play?" This will enable us to understand more clearly how the idea *exists.* In answering the question it is essential

to note immediately that the *significance* which departs from, refers beyond, transforms the immediate image, finds its function in conducting us back to concrete experience. Knowledge does not consist in framing copies of extra-mental existences, but in developing the function by means of which we are carried from one form of experience to another. The 'objectivity' of the function consists in the fact that although in one sense the knowledge function and its termini are absolutely individual and 'subjective,' they are at the same time and equally universal. In other words, there can be a subjective synthesis for us only because we lay bare a deeper, yet immanent order which constitutes the inner reality of the movement which is our experience. For the principle of this distinction we must thank Kant and his followers of the positive school. Its working out has not yet been fully accomplished. In it, for example, we find the strong and weak points of Mr. F. H. Bradley's system. Underlying this thinker's philosophy is a definite view of knowledge and of its relation to immediate experience. According to Mr. Bradley every idea is a transformation of immediate experience. Knowledge thus rises above and departs from the actuality of given reality. The more determinate ideas become, the more difficult therefore is it to understand how knowledge can be other than a mutilation of the real. Mr. Bradley recognizes fully that the emphasized image or 'that' is stripped of its individuality when it enters into the idea-function. 'Content' may embody the most determinate forms of meaning, but nevertheless it has deprived 'existence' of its own nature and has imposed another and arbitrarily, as it must appear, unless we can carry the investigation one step further. This difficulty even the average Neo-Kantian has failed to recognize. He still trades with the thought that universal and particular, identity amid differences, are necessarily correlative distinctions within knowledge. The root of the matter, viz., that the meaning which is identical in both particular and universal, differing only in its use or appearance, is a transformation of immediacy, which must appear as arbitrary unless the complete function of knowledge can be laid bare; this root, I say, escapes him. Not until we realize that the idea function which separates us from the immediacy of experience aims in the same movement at connecting us with immediacy can the difficulty be removed and knowledge be given a legitimate work to accomplish. To illustrate this point let us analyze the concrete judgment: "That is a crayon." In our vison of the crayon the 'that' is embodied in the factors of brightness and color fixated by our gaze. As a factor in the complete judgment the 'that' therefore

is constituted by the image plus the initiation of the transformation which constitutes the 'what' or meaning. The 'what' in its turn embodies the references of the image or more specifically the anticipations of other possible concrete experiences, other 'thats'—sensations of pressure, of sight, etc., and also of the activities through which these possible experiences may be actualized. The idea is therefore a transitive state which in its movement controls the experience of definite substantive states.

Ideation as reference is embodied in determinate forms of activity which in their initiation and discharge implicate the concrete imagery which furnishes the base for each discharge and illumines its course. The projection of images which indicate the immediate experiences to be realized constitutes our foresight or self consciousness; the lines of discharge constitute the conditions under which the concrete experiences may be realized. Looked at as a psychical event, the idea, therefore, is always a symbol which operates upon a basis of imagery. As a symbol the idea embodies the self-conscious references or relationships which constitute on the one hand our exact anticipations of possible experiences and on the other the necessary conditions under which our anticipations can be realized. For this reason ideation not only is but must be self-conscious and foreseeing. But again, our foresight or anticipation is embodied in imagery whereas the governing conditions are constituted by determinate habits of discharge. The conclusion of our earlier investigation is thus sustained. Imagery and ideation cannot be separated from each other. The thought function includes both. In the initiation of the mental discharge imagery is entirely subordinate; in the process of discharge it comes more and more to the fore, until finally its clear and determinate character constitutes the anticipation which makes knowledge a matter of intelligent direction in life. If, further, we turn attention to the part played by ideation we realize at once the true character of the criterion of knowledge. This does not rest in the vividness or intensity of the involved imagery, as some have supposed, nor again in the consistency of ideas one with another. These have their value, but that value is not fundamental. Intensity, vividness, clearness of imagery give a 'realizing' sense to our pale conceptions but this may be a source of illusion, *e. g.*, in our dreams. Consistency is of extreme value in the manipulation of ideas already formed and in the framing of new combinations from old material. It does not assist us, however, in the original determination of ideas. The natural test of knowledge rests in an inquiry into what the idea attempts to per-

form. As a symbol, it professes to set before us the conditions upon the fulfillment of which certain determinate experiences may be realized. The criterion of the truth of the idea, therefore, is: "Does it realize its pretensions?" Of this daily experience and the work of science are proofs. And at this point we may recur briefly to the treatment of 'objectivity' in knowledge. According to the remarks made immediately above, an idea is objective when it realizes its anticipations through their appropriate conditions. This is true universally. There are many kinds of knowledge and many spheres of existence, but in every case reality is distinguished from illusion on the basis of the reliability of our intellectual symbols. In connecting this with previous statements concerning 'objectivity' it will be sufficient to point out that in the development of symbols it has been discovered that mere subjective wish or whim must be subordinated to the discovery of a method of framing anticipations and of determining conditions which are given to us through sense experience and are not arbitrarily imposed by us. To the discovery of this determinate order latent in every aspect of existence science universally directs itself. And, as Kant pointed out, in the recognition of this immanent order as the reality of every existence is found the true answer to Hume's question concerning the necessary and universal significance of ideas. Experiment thus remains as the natural and ultimate test of knowledge. Having formulated our anticipations and wrought out the corresponding conditions to the best of our information and experience, the last step remains to find whether the conditions when set in operation realize concretely the expectations which we have framed. If such experiences are realized for us as we expected, and if they are realized after the manner anticipated, our ideas are true and real, knowledge is attained. If our expectations fail, then in the sense in which alone knowledge has meaning for us, the idea is false. With this result science agrees, as through the experimental method and that alone it has built up its great fabric.

We have now examined the different aspects of the structure and function of ideas. Meaning, whether embodied in abstract universals or in concrete particulars, has turned out in each case to be symbolic. Symbols in their turn have turned out to be principles of anticipation and control of concrete experiences. Knowledge has a distinct and objective function, but that function never consists in furnishing us with copies good or bad of an extra-mental reality. Its function is rather that of bringing to more determinate consciousness the reality which is already in mind although in an indeterminate form. Put

otherwise the function of knowledge consists in developing reliable symbols of control in the anticipation and manipulation of possible experiences. Thus, on the one hand knowledge is relative and on the other it is objective. Judgments are alike hypothetical and categorical. Every fruitful idea contains essentially a condition and also a determinate expectation. As relative, knowledge grows and differentiates with the development of reflective critical experience. It does not profess to set forth the complete and possible content of reality but to determine the regulative principles of the varying aspect of reality. As objective, it places the criterion in the control of anticipation by actual conditions.

We can perhaps close this essay best by squaring accounts with association and transcendental interpretations of ideation.

The associationist asserts that the process of ideation can be resolved into a series of images related together in determinate forms. And from one point of view this interpretation is entirely correct. If we are looking toward the reproduction of meaning *already established* we must analyze it into substantive states, dynamically connected. If, however, we ask ourselves how meaning was originally established and what it does functionally at any moment, we must maintain unreservedly with the transcendentalist that association does not account for the origin of meaning and that it is no more than a record of meaning previously developed. Meaning involves a consciousness of the unity present in all relationship. Fundamental therefore to all significance is that synthetic activity which makes the function of reference possible. The mistake of both schools has consisted in tearing into two the single process of ideation and in setting up the mutilated parts as though they were each the living whole. Ideation involves the presence of both analysis and synthesis: the development of symbols universally takes into account the reproductive results of past gains. Imagery and reference, terms and relations, cannot be divorced. And this is the final word of insistence—only when we realize the presence of imagery as the embodiment of symbols, and of reference as the spirit of control for imagery, can we understand the real nature of ideas and keep clear of the rocks on either hand which have wrought such woe to thought.

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PSYCHOLOGICAL LITERATURE.

Les Dilemmes de la Métaphysique Pure. CHARLES RENOUVIER. Paris, Félix Alcan. 1901. Pp. 283.

The evil plight of metaphysics, where every assertion is met by a counter-assertion and where disputes seem never to be decided because there are no principles accepted in common by the disputants, has long been a favorite theme with the sceptic, triumphing in the proof that everything in this region is sophistry and illusion, and with the philosophical reformer, urging his new method by which metaphysics was to be brought into the sure path of science. But in spite of the confident denials of the one, men still continue to speculate, impelled presumably by the ineradicable metaphysical need inherent in human nature; and in spite of the explanations and hopeful predictions of the other, the contradiction in principles was never perhaps more acutely felt than now. Not the least important undertaking in philosophy is the attempt to reduce the many contradictions to a certain few fundamental contradictions and to discover, if possible, the ultimate logical presuppositions from which they spring. This is part of what is attempted in the work before us; the other part is to suggest the way of escape.

The fundamental contradictions that underlie all systems of pure metaphysics are, in Renouvier's opinion, the following five: (1) The assertion of the unconditioned—there exists being *in se*, unconditioned, necessary and unknowable, to which is opposed the assertion of the conditioned—there is no being *in se*, but all that is is relative to phenomena and to the laws of our cognition; (2) the assertion of substance—there exists *in se* an indefinable and unknowable entity, the seat of qualities, to which is opposed the assertion of law, or function of phenomena—substance is nothing but the logical subject of qualities and definable relations; (3) the assertion of the infinite—there are concrete quantities, *e. g.*, the series of events in time and of objects in space, the really distinct parts of which do not make up numerically determinate wholes, to which is opposed the assertion of the finite—what is really composite, and hence the actual series of past events, the world of objects in space and the whole world of beings generally, consists of a finite number of really distinct parts; (4) the assertion

of determinism—every phenomenon is completely determined by the given fact of its antecedents and circumstances, to which is opposed the counter-assertion of freedom, which denies this; (5) the assertion of things—consciousness and persons are products of the world, to which is opposed the assertion of persons—consciousness is the principle of both knowledge and being, and the world is the product of persons.

Now these antitheses constitute, as they are here represented, genuine dilemmas, for the opposed propositions, it is contended, are in each case mutually contradictory; neither can be refuted by principles admitted in the same sense by the partisans of both, while yet, being true disjunctions, choice between them is inevitable. It is further contended that the successive propositions mentioned first in order, when taken together, form, with their corollaries and consequences, one logically coherent system, while those opposed, with their corollaries and consequences, form another. And inasmuch as the latter, the theses of the conditioned, law, the finite, freedom and persons, make up the framework of Renouvier's own system, there is the implication that ultimately we are forced to choose between that and every other. The logical principles from the acceptance or rejection of which, according to Renouvier, these contradictions spring, are the principle of relativity, which asserts that no knowledge is possible or existence conceivable except through relations, and the principle of contradiction, or the rule of the agreement or disagreement of relations respecting the same subject. The first principle is rejected, and the second also, so far as it implies the first, by the advocates of the propositions on the one side, the realists and necessitarians, while it is the very foundation principle of their opponents, the phenomenists and libertarians. The solution of ultimate questions on pure grounds of logic is thus declared to be impossible. So much Renouvier concedes to the sceptic. What then does he propose as the philosophical reformer? Simply, the free choice of fundamental principles. Freedom is thus presented, not merely as a practical belief, the foundations of which may be invalidated by science, but as a rational belief, which serves as a principle of theory and of all knowledge.

Renouvier has written nothing better or more characteristic than this little book. It is in its way a philosophical masterpiece, one well suited to introduce our students to a school of thought that has long been influential in France, but which has hitherto received little attention here, in spite of the public acknowledgement of his own in

debtedness to it made by our most distinguished American psychologist. And Renouvier, it may be added, is worth the study, not least perhaps because he so often compels us to disagree with him, a philosopher subtle as Lotze, stimulating as Schopenhauer, clear as Descartes. Effective criticism of a work like this involves discussions that would here be out of place. The psychologist will probably find a point of interest in the author's rejection of a soul-substance on metaphysical grounds, especially in its connection with the assertion of personal freedom, a connection which, if admitted, would seem to involve important consequences alike for the psychologist and the man of science generally, consequences subtly suggested by the dedication of the book to Boutroux, the author of the 'Contingency of the Laws of Nature.'

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La Mémoire. J. J. Van Biervliet. (Bibliothèque internationale de psychologie expérimentale, normale et pathologique.) Paris, Octave Doin. 1902. Pp. 346.

The purpose of this book is, in the words of its author, 'to sketch the history of the problem of memory and its evolution during the last fifteen years.' He declares that he 'will glide over the various recent theories, however ingenious they may be, in order to dwell upon clinical observations, and to treat at length the research work undertaken in the various laboratories of experimental psychology.' By way of further limitation, he adds that he 'does not wish to make a complete critical catalogue of all the recent work, but to give a sufficient knowledge of the actual movement to prepare the reader to undertake or simply to comprehend original researches on memory.' Herein one may detect a conflict of aims that has led to a book missing somewhat either mark. On the one hand the reader can scarcely feel assured that he is made sufficiently well acquainted with the work done in any special field to undertake forthwith an investigation calculated to advance our knowledge thereof, while he who hopes merely to be able to comprehend works of original research may find so many elaborate accounts of experimentation a trifle tedious. Furthermore, a novice in these regions will feel aggrieved that the limit of fifteen years has shut out anything save the barest mention of the work of Ebbinghaus and any notice whatever of that of Wolfe.

Professor Van Biervliet's book, however, is well timed and will serve the useful purpose of furnishing a transparent and fairly well

selected account of recent experimentation on memory and observations upon its derangements. He uses as the basis for the divisions of his treatment the well-worn analysis of memory into retention, recall, and recognition, the last including localization. However excellent this may be as a classification of the factors in memory, one can hardly fail to be at first glance a little skeptical about the success of its application to a collection of studies most of which are complex enough to involve some sort of suggested conclusions about nearly everything connected with memory at all. I am bound to confess, however, that in M. Van Biervliet's hands it has succeeded far better than I had anticipated, and by it he reduces a heterogeneous mass of material to at least the appearance of order. To suppose that we have more than this may, however, lead to questionable conceptions. When, for example, experiments on the power of children to reproduce immediately a series of numbers or words or the like are classified as dealing with the intensity of the memory of fixation, one is doubtful whether the results may not be due to such elements as habit of attention, familarity with material, etc., quite as much as to mere retentive power. In this event the experiments belong under the division dealing with association with as much justice as experiments on the formation and breaking of habits, which are thus classified by M. Van Biervliet. Indeed, except where there is undoubted destruction of cortical tracts with complete loss of specific images dependent thereon, we are offered by the author no clue whereby we can determine in how far the forgetting of a thing is due to failure to retain rather than to a failure to associate. The experimentation cited tests generally, I am prone to think, the permanence of associations instead of any retentiveness separable therefrom.

M. Van Biervliet devotes nearly one-half his book to 'the memory of fixation,' dividing this part into three chapters, which deal respectively with the seat of memory, its types, and the intensity of the memory of fixation. In the first of these, while discussing retention as a general function of matter and particularly of the human body, he affirms roundly the existence in the germ of memory for the habits acquired by the parent, and ignores silently the doubts and arguments of the Neo-Darwinians. He then sketches rapidly the development of the theories on the localization of functions of the brain, and passes to a somewhat more detailed discussion of the history and present state of our knowledge of the nervous apparatus involved in using language. He notes that motor aphasia can be separated from agraphia, and amusia from aphasia. The second chapter, on the types of memory,

leaving the work of Galton unmentioned except for a minor contribu tion, begins with the classification of Charcot, and suggests that types may be based not only on the sense involved, but also on social rank, race or sex. The bulk of the chapter is occupied by reviews of the experiments of Cohn on the coöperation of acoustic-motor and visual memories, Toulouse's examination of the imagery and methods of remembering used by the novelist Zola, and Binet's book on great calculators and chess players. The importance of the motor element is emphasized, and its relation to the predominantly visual type on the one hand and the auditory type on the other is well brought out. The author remarks the complexity of the elements involved in most individual memories, and shows how certain of these are often strengthened because of their use for professional purposes. The third chapter reviews experiments on the effects on power of recall of race, age (children being tested), repetition, distraction, rhythm, etc., with studies on the memory for shadows and distances and of muscular memory for length of lines. Smith's experiments on muscular memory are here chronicled, though differing little from those of Cohn, which are dealt with in the preceding chapter.

Part Second, on recall, has two chapters, the first of which takes up the subject of maladies of the reproductive memory. Here we are concerned chiefly with the disappearance and reappearance of groups of memories and the phenomena of multiple personality. A second class of cases, where there is progressive destruction of all associations, is also discussed. Experimental researches on association are taken up in the second chapter. Most stress is laid on the work of Bourdon and Aschaffenberg, though results reached by Bergström, Jastrow, Miss Calkins, Ziehen and others are mentioned.

Part Third discusses recognition, and localization of memories in time. It follows the general plan of dealing first with pathological cases and then with experimental work. Some most interesting material on the false recognition of things experienced for the first time is introduced. The cases range from that occasional feeling so common among us of having mysteriously experienced *just this* before, to an instance where the whole psychic life seemed to be a duplication of the experience of a preceding year or even a reduplication of that duplicate experience. The chapter on experimental work deals with five contributions, but devotes most space to Bourdon's work on the recognition of repeated letters in a series and Vaschide on the localization by memory of the words in a series.

In conclusion, along with various precautionary suggestions as to

method, the author expresses particular distrust of results reached with trained psychologists as subjects. As a parting word, he holds out the hope of a rich reward to the experimenter on memory in the practical application of his results to education, and notes the so-called 'word method' of teaching reading as an illustration of this.

In spite of an endeavor to represent impartially the work done in the various laboratories of the world, the contributions of the French are emphasized somewhat unduly. More than half the writings that are given anything more than mention are in the French language, while nearly half the authorities cited are French. Moreover, while we might expect them to receive the lion's share of the part devoted to discussion of pathological cases, we should hardly think their laboratory work would require decidedly more space than that given to others. It is to be regretted that such studies as those of Münsterberg on habits, Bryan on motor ability, Netschajeff on the memory of children, and Lay on mental imagery should have been unnoticed; also that there should have been no mention of what has been done on memory for tones and on the earliest memories of childhood. Moreover, while there has been but little experimentation on the quantitative and qualitative effects of considerable intervals of time on our memories, the importance of this field makes it deserving of a special mention such as M. Van Biervliet does not give.

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BINAURAL HEARING.

Ueber binaurales Hören. Gino Melati. Philosophische Studien, XVII., 431.

The above contribution from the Leipzig Institut commends itself at once to the reader as a report of well planned and carefully executed experiments. The chief object of the investigation was to submit to a fresh and particularly rigid examination the phenomena of binaurally perceived beats. May these occur when every precaution is taken to exclude the transmission of excitations from one ear to the other either through the air or through the bones of the skull? And if beats may be heard under these conditions, how do they differ from those heard monaurally? In the process of answering these questions several collateral matters were incidentally investigated also.

The desired conditions were secured first by the use of three separate rooms, the subject being placed in the middle room and a tuning-fork in each of the others. The tones from the forks were conducted

to the middle room through brass tubes, in the ends of which were inserted pieces of glass tubing which could be slid in and out and thus be inserted in the subject's ears. The forks were of 500 vibrations and were electrically actuated. The tone was thus continuous, and by means of the adjustments used could be kept constantly at any grade of intensity desired. The pitch of either fork could be varied by very acurately adjusted running weights. Every precaution was taken, by means of paddings and wrappings, to exclude extraneous sounds, and the reader readily shares the author's conviction that only the pure tones of the forks reached the subjects' ears.

But now the conditions which excluded the transmission of sound from one ear to the other. Transmission through the air was easily excluded by using tones so weak that no sound could be heard by either ear when the glass tubes were not inserted in the ear passages. Transmission through at least the outer bones of the skull was held to be excluded, partly because tones of this low intensity could hardly possess energy enough to carry vibrations over the entire surface of the skull, but particularly because of the introspective testimony of the observers on two points. First, if both tones be sounding and one ear be stopped by the insertion of the finger, no beats must be heard, under correct conditions. If heard, the intensity of the tones must be still further reduced until the beats vanish. Secondly, one ear being closed by the finger, and the tone upon that side being then interrupted, the subject is asked to note any change in the tone heard. If the reply is given that the tone has become less full in character, this is taken as evidence that the influence of each sound is confined to the ear upon its side.

The desired conditions being satisfactorily secured, and their maintenance carefully and frequently controlled, the four trained subjects were called upon to note every possible phenomenon and to make constant comparisons between the binaural and the monaural experiences. The experiments were begun with mistunings of the forks which produced from 2–15 beats per second. In later experiments the difference between the vibration rates was much further increased. The particular results in connection with the perception of beats may be briefly stated:

Binaural beats are less definite in character than the monaural and are less clear. They are more subject to fluctuations of the attention.

Binaural beats are of less intensity than the monaural, the maximum intensity occurring when the beats are less than 10 per sec. The law holds that as they increase in frequency from this point they

decrease in intensity. Consequently, the point of greatest roughness is not, as in monaural hearing, at 30 beats per second, but rather in the region of 2–10 beats.

The extreme limit of binaurally perceptible beats is, in this region of the scale, at 50 per second, against 60 or 70 for monaural hearing.

The feeling of roughness is never so great in binaurally as in monaurally perceived beats, and it disappears entirely when the interval between the tones exceeds 30 vibrations.

Several further points, for the observation of which the experiments afforded peculiarly good opportunities, were carefully noted:

1. All the intervals employed were felt to be dissonant, though, contrary to Stumpf, in less degree than in monaural hearing. But the fact to be noted is that the dissonance remains beyond the point—30 beats per second—where the roughness has disappeared, and as clearly also in those cases where, with the tones just at the threshold of perception, all roughness and all beats too are wanting.

2. A subjective tone will beat with an objective tone. Stumpf denies this, but the author has the opportunity to observe this upon himself under carefully controlled and varied conditions.

3. Again, in direct opposition to Stumpf, the author finds that the grade of fusion of the binaurally heard tones diminishes as the interval between them increases. The degree of fusion is never so great as in normal hearing, and there is simply no fusion at all when the tones are at the threshold of perception.

4. The claim made by LeRoux, Urbantschitsch, Bloch and others, that binaural tones exercise upon each other a mutual reinforcement, was submitted to a thorough examination. The result found was that when the interval between the two tones is small, the intensity of each is certainly increased by the presence of the other. This is not the case, however, when the vibration rates of the tones differ by more than 10 per second. Then, the author states, one may rather speak of a mutual weakening of the tones. Bearing in mind the low intensity of the sounds employed, one must suppose that this mutual reinforcement of tones is centrally conditioned.

5. A few observations were made on the localization of the various impressions. Each of the binaural sounds was found by careful scrutiny to lie, not exactly in the ear on its appropriate side, but slightly within the head. Under these circumstances the beats were localized in the deeper intracranial region between the binaural impressions. When, however, the intervals were greater than 15 vibrations per second, the beats seemed to be located at some far distant point.

Schäfer's well-known observation to the effect that the closing of one ear will change the localization of a tone just perceivable by the use of a resonator, so that the tone wanders from the stimulated ear to the middle of the head, is subjected to an interesting criticism. The author suggests that the apparent shifting of the sound may be illusory, due to the disturbing influence brought about by stopping one ear. Such an illusion disappears with practice, and the author is convinced that a weak tone is invariably located within the head, whether the unstimulated ear be closed or open. To show the great reliability of his results, he appeals to the fact that the arrangement of his experimental conditions allowed a greater convenience and certainty of observation than in Schäfer's case. It is highly desirable that further experiments be made in this extremely interesting field of intracranial localization.

As remarked before, the whole article, the main points of which are summarized above, gives the impression of stating results and observations that are more than ordinarily reliable and worthy of attention.

A good historical survey precedes the report of the experiments.

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THE VISUAL PERCEPTION OF SIZE.

Die Form des Himmelsgewölbes und das Grösser-Erscheinen der Gestirne am Horizont. VON ZEHENDER. Zeitschrift für Psychologie und Physiologie der Sinnesorgane, Vol. 24. Pp. 218–284.

This elaborate paper contains valuable historical matter and a reopening of the whole question as to the vaulted appearance of the sky, and the apparent enlargement of the sun and moon near the horizon.

The historical parts of the paper, however, make no claim to completeness, and in fact Lipps's theory that binocular vision is an important element in determining the appearance of the heavens is not referred to. The author goes back to Malebranche, and also devotes a special appendix of considerable length to a curious *Promotionschrift* by Treiber, in which, as early as 1668, the vault-appearance of the heavens is attributed to mist and clouds forming an actual concave overhead, concentric with the earth's surface; and by computation the altitude of this vault is set down by him as one-tenth the radius of its base.

The author's own view on this first question is, in the main, the same as Treiber's. Clouds and mist are the prime source of the vault appearance; without these there would be no direct impression of vaulting whatever. The clear blue sky, he holds, is of itself formless, although indirectly, as a whole, it gives a vague sense of rotundity, in part carried over perhaps from observing the clouded sky and also due to our natural tendency to represent infinite space as a sphere. And, again, the starry sky suggests to the author a formless swarm of points, and not a vault or surface. The low vault of the clouded sky is consequently no illusion, but is the truthful appearance from below of the concave of vapor concentric with the earth; knowing the height of the stratum we can compute the actual form of the sky. Evidence that mist and vapor alter the apparent form of the heavens is gained from careful quantitative experiments by von Sicherer on the bisection of the apparent arc from zenith to horizon. These results show that in dividing the arc by eye, one puts the mid-point nearer the horizon in cloudy weather (37.7° high, on the average, as against 42° for cloudless days) indicating that the arc seems depressed when the sky is overcast.

On the second problem—the illusory change in the size of the sun and moon—the author is sure that this is not to be explained by the occasional low-vault appearance of the sky. He does not seem so certain of the positive explanation. Several factors are dwelt on; and first, the greater atmospheric clearness toward the zenith, which makes the object seem nearer and consequently larger (although the author also cites without reconciliation Zoth's questionnaire results, showing that for most persons the sun and moon actually seem farther off at the zenith). He also feels that the effect toward the horizon may have some connection with the well-known over-estimation of an extent that is minutely subdivided. That the direction of the line of sight is also an important factor is shown by his summary of Zoth's and Stroobant's experiments on the comparison of lateral extents on a level with the eyes and overhead. The evidence is clear that extents overhead seem smaller.

But perhaps the main emphasis is laid on what the author believes to be a predisposition to regard universal space as spherical, to which I have already alluded. This tendency, it seems, leads us also to view lines that are perpendicular at the horizon as great circles of a sphere and as converging upward to the zenith. The sun and moon became involved in this system of imaginary meridians, and out of it all, in a way that is not made entirely clear, comes the illusion of diminished

size in the high heavens and of enlargement toward the horizon. The author feels that there is probably some connection between these facts and the observation of Volkmann that perpendiculars, if they are to seem parallel, must be made slightly convergent above.

The author's discussion of the shape of the heavens has in it much acute and convincing observation. After reading his account it is easy to satisfy oneself that the heavens are of decidedly different form according to lighting and atmospheric conditions; clouds especially make the vault seem low. And the cloudless sky by day, apart from the region of almost inevitable mist toward the horizon, has certainly a much less definite and perceptible form than the cloudy sky. But the present writer cannot confirm the author's statement that the clear night-sky shows only a formless swarm of stars. I get a decided impression of vaulting from the starry sky; and a number of persons, questioned in the evening with the facts before them, are unanimous that they similarly have a definite vault-impression. For myself, on especially clear nights, the impression has been of a *high* vault, the stars overhead appearing farther off than those below. Others, and I myself at other times, see the evening sky more as a hemisphere.

As to the low-vault appearance of the clouded sky, it does not seem certain that the author's explanation is correct. It seems hardly probable that the curvature of the earth can have much to do with the matter. In the first place, the curvature of the cloud-vault often seems much greater than that of the earth; whereas on von Zehender's theory it should, if anything, seem less, since the cloud-concave has a larger radius. And, secondly, the apparent concave can in all probability be fully explained as a peculiar instance of perspective. Any one who will observe the wall or ceiling of an exceeding long building, viewed from the middle so that the same plane runs to vanishing points both before and behind, will notice that the plane appears slightly to curve around the observer as it sweeps from one vanishing point to its polar opposite. The farther off one can get from the plane or line observed, the greater must be the apparent curvature as it passes the observer; thus the earth's surface even when viewed from a slight elevation seems to curve *upward* to the horizon, rather than downward as the actual curvature would lead one to expect. And let the eye be farther off, as when viewing the sea from some high headland, or from some mountain near the coast, and the apparent concavity of the surface becomes very striking; the waters mount upward toward the horizon and become veritably the 'high seas.' Similarly the cloud-stratum, viewed as it usually is from a con-

siderable distance—often several miles beneath—ought, on mere principles of perspective, to show the vault which our author and others would ascribe to a real curvature. They are merely bringing in a dubious factor, *præter necessitatem.*

The author's account of the sun-moon illusion is convincing, it seems to me, in the point that the illusion is not due to the low-vault appearance of the heavens; for it appears when the heavens do not seem low-vaulted. And he is probably on firm ground when he attributes the illusion to various factors. The least satisfactory part of his account is where he introduces the idea of converging meridians and Volkmann's illusion of perpendicular lines. If perpendiculars that are really parallel seem divergent above, would not this make the sun-moon illusion more difficult rather than easier to explain? For Volkmann's illusion by itself would lead one to expect that a disc of a constant size would seem larger the higher it went. And the suggestion of converging meridians does not seem more helpful. For assuming that the span between some given pair of meridians is taken as the unit of measurement, this unit is largest at the horizon and should as a result make the sun's disc seem small; and similarly the disc near the zenith where the lines converge would overlap more of these units than at the horizon and should therefore seem large, instead of small as it actually does. The more promising 'lead' in this problem appears to be found in the results of Zoth and Stroobant, showing that the direction of the line of regard influences our feeling for extension, so that of the equal distances, one looked at horizontally, the other from beneath, the latter seems the smaller. But even after all the objections that can be raised to particular points in his paper, one must gladly acknowledge that von Zehender's discussion of these two problems is by far the most illuminating that has ever been given.

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Ueber die mechanischen Correlate von Raum und Zeit, mit kritischen Betrachtungen über die E. Hering'sche Theorie vom Ortssinne der Netzhaut. E. STORCH. Zeitsch. f. Psych. u. Phys. d. Sinnesorgane, XXVI., 201–226.

In a fragmentary discussion of two pages, the mechanical correlate of time is asserted to be the continuous material changes of the brain, corresponding to the not further analyzable, most primitive activity of consciousness, which consists in the fading of each moment of a sen-

sory impression, even while the stimulus persists, into a memory image. This activity is immediately apprehended as the not further explainable *Time*, which is thus shown to be a necessary attribute of every sensation, even the first.

The mechanical correlate of space is a movement moment which is superadded to every sensory impulse. The psychic correlate of this movement moment is not to be confounded with the sensations of movement, since it never appears in consciousness by itself, and has absolutely nothing sensory in it. The author develops this theory of space without reference to its previous formulations or its classic difficulties, on the basis of a thorough-going psycho-physical parallelism. He supports it by an interesting and important case of monocular double vision without any physical cause. The patient developed strabismus in childhood. At nineteen the right normal eye was enucleated, and the patient reported that about 5° from every point fixated a second or 'illusory image' of the object appeared. Ophthalmoscopic investigation demonstrated that when this illusory image was fixated the retinal image fell on the anatomical macula, although it seemed to the patient that he was then looking beyond the real object. With the strabismus there had evidently developed an artificial macula. The author explains the double perception from a single retinal image by the development of a new system of local signs, in consequence of the shifting of the center of movement, while the original local signs of normal vision still persisted. A retinal image falling on the artificial macula is thus referred both to the center of vision and to a point at a distance from it, corresponding to the distance between the real and the artificial macula. The case deserves to be carefully followed and fully described.

The article is unfortunate in several respects. The only reference to any previous discussion of time is a misquotation of Kant, in which he is made to call time and space—'Formen reiner Sinnlichkeit' instead of 'reine Formen der Sinnlichkeit.' This is doubtless merely a slip, but it is an unfortunate one, for it looks like a total misunderstanding of Kantian terminology. Even more unfortunate are the general indifference to previous discussion and the thoughtless assumption and positive statement of debatable hypotheses, of which one at least, viz., the existence of a quantitative, non-sensuous consciousness of movement, has been proven false both by experimental investigation and by introspective analysis.

RAYMOND DODGE.

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On the Psychology and Physiology of Reading. II. EDMUND B. HUEY. The American Journal of Psychology, XII., pp. 292–312.

The article concludes the report of studies interrupted early in 1899. It contains a table showing the number of words read during one fixation in lines of varying length; and an interesting account of the rate of reading under various conditions of reproduction. The discussion of the unit of reading is a restatement of the theory of successive apprehension, agreeing in the main with the theory of Exner, without any consideration of the difficulties that theory has been shown to involve. The short account of the interpretative process of reading contains valuable introspective data.

RAYMOND DODGE.

The Perception of Visual Form. MISS L. HEMPSTEAD. Amer. Journal of Psychology, Jan., 1901. Pp. 185–192.

This paper is a preliminary report of experiments on the perception of gray outline figures placed on gray cards. These figures were seen through slits in a rotating disk which so controlled the illumination that figure and background were just perceptibly different from each other. Observers perceive such figures incorrectly, adding lines to complete symmetry and to fill out what seem to be, in many cases, purely individual constructions. Angles are seen much rounded and parts of the figures are vague, some lines being entirely omitted.

The writer evidently sees that these preliminary experiments have not dealt successfully with the distinction which will be of crucial importance for the interpretation of these results, namely, the distinction between the sensory factors which arise under the complex conditions here presented, and the perceptual processes resulting from these sensations. Later treatment of this phase of the problem is promised.

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Études de Psychologie. J. J. VAN BIERVLIET. Paris, Félix Alcan. 1901. Pp. 201.

The writer has presented in a series of popular essays his views on the structural and functional asymmetry of the human body, on the nature of the Müller-Lyer illusion, and on Flournoy's illusion of weights. He has also added an account of some reaction-time experiments tried in Wundt's laboratory in 1893. The book contains a few short tables of measurements, but is otherwise merely a restatement of well-known facts and conclusions. The chief significance of the

work is to be found in the concrete exhibition which it gives of activity in the psychological laboratory at the University of Gand.

CHARLES H. JUDD.

THE PERCEPTION OF MOVEMENT.

La perception des mouvements par le moyen des sensations tactiles des yeux. B. BOURDON. Revue Philosophique, 1900, L., 1–17.

Does the Sensation of Movement Originate in the Joint? W. B. PILLSBURY. Amer. Jour. of Psych., 1901, XII., 346–353.

A Comparison of Judgments for Weights Lifted with the Hand and Foot. A. J. KINNAMAN. Amer. Jour. of Psych., 1901, XII., 240–263.

Bourdon discusses the delicacy of the perception of movement with the eye when the latter fixates an isolated moving object. If a single luminous point is visible in a dark room, distant one-half meter from the eye, and is fixated as it moves, its movement begins to be perceptible with a rapidity of 2 mm. (14′ of arc) per second; the perception is sure with double this rapidity. The perception of the movement of larger luminous objects is slightly less delicate, because of less exact fixation. Intensity of illumination, and number of objects moving together, make no difference. When, however, a point moves in a field of visible immobile objects, the perception of its movement is much more delicate, being sure with a rapidity of 0.15 mm. (62″) per second. Aubert explained this difference by the assumption that a comparison is necessary between moving and not-moving objects, in order to give rise to the perception of movement; and that when the moving object alone is visible, it is compared with a representation of immobile space. Bourdon finds no trace of such a comparison when the experiment is actually tried. He concludes that the sensations giving rise to the movement-perception originate either in the eyelids or in the eye-muscles, or in both. That the eyelids play a large, perhaps preponderant, part is established by a series of experiments. By means of suitable apparatus a uniform movement of eyelid over eyeball was produced; the direction of movement was clearly distinguishable with a rapidity of displacement of 0.25 to 0.375 mm. per second, equivalent to an angular displacement of 1.19° to 1.79°. This sensibility is but slightly reduced on rendering the cornea anæsthetic. The tactile sensibility of the eyelids is not very delicate; a distance apart of 10 mm. is requisite for distinguishing two contacts. But other parts as little sensitive in this manner, as for example the

back of the hand, yet possess a very delicate sensibility for movement, where there can be no question of muscles involved, but where the skin can be moved freely over the underlying parts. The eyelids move to right and left, as well as up and down, in company with the movements of the eye; and their delicate perception of movement probably arises from the consequent distension of the skin. The muscles play an important part also, but this Bourdon has not investigated. To the argument of Volkmann that muscular and tactile sensibility of the eyes are not sufficiently delicate to account for our perceptions of movement and position, as proven by the fact that with eyes closed, we are very uncertain as to their position, Bourdon answers that the inexact convergence of the eyes under these conditions introduces some confusion; that there is a rapid diminution in tactile sensibility here as elsewhere when a given position is maintained; and that the perception of position is fairly exact when a single movement of the eyes and an immediate observation of the direction of regard is made.

"Since Goldscheider's work on the 'Muscle-sense' there seems to be very general agreement that the joint is the only or by far the most important source of the sensations that inform us that we have moved the members of the body." This view is now disputed by Pillsbury. The delicacy of perception of movement about the elbow and knee is diminished almost or quite as much by rendering anæsthetic by means of an electric current the wrist or the ankle, as by currents through the joints in question. This fact, together with the lack of anatomical evidence that the joints have sensory endings, makes it probable that the sensation of movement is derived mainly from the tendon and muscle rather than from the joint, inasmuch as these and these only are equally affected when the current is applied in the two places. It is also shown that the effect of the induction current in diminishing sensitivity cannot be due to any distracting effect produced by it.

Kinnaman's article gives the results of 9,000 out of a total of 18,000 tests made in judging weights lifted with the hand and with the foot. His standard weights were nine: 100 grams, 400, and by intervals of 400 up to 3,200 grams. They were lifted by aid of a band of cloth into which was inserted the hand or foot. This arrangement secured a more gradual lifting of the weights than the more rigid apparatus ordinarily used, and gave considerable prominence to tactile sensibility in case of the lighter weights especially. His more important results are the following: (*a*) The weights were discriminated a little better through the hand than through the foot; (*b*) the relative

difference was greater for the small than for the large standards; (*c*) both showed a lower degree of sensibility both above and below 2,000–2,400 grams, or one-third of the reagent's maximum lift. The curves show at first a rapid increase of sensibility to about the 800-gram standard, followed by a less marked increase up to 1,600. Then they rise rapidly to the maximum, after which a slower decrease sets in. (*i*) The second test of the series is judged better than any others.

Among the introspective observations noted by Kinnaman are the following: (*b*) The judgment seemed to be based more on the changes in stimuli that occurred at the beginning of the lift, than on any constant sensations after the weight once cleared its base of support. The best results were attained by directing the attention to the first sensations. The basis of sensation appears to be the memory of a former change of sensation as compared with a present changing sensation. (*g*) One can distinguish quite noticeably, when lifting very large weights, that he is beset with a focal sensation, and with numerous marginal sensations, some having only a distracting effect upon the attention, such as noises, heat, the pains from uncomfortable positions and muscular pains, and others that must be regarded as auxiliary sensations to the focal sensation. They, with the focal sensation, figuratively, in a heap, constitute the real basis of discrimination. Among these marginal auxiliary sensations the following were pronounced: (1) Touch sensations around the grasp; (2) pressure sensations; (3) changes in the pressure of the back against the chair; (4) increased pressure on the seat of the chair; (5) feet pressing more firmly on the floor; (6) other hand putting forth effort; (7) interference with breathing; (8) interference with circulation; (9) intercostal and abdominal muscular sensation.

These marginal sensations are considered by Kinnaman to be of large importance in judging weights. The conscious basis of judgment was tactile for the 100-gram standard. The marginal auxiliary sensations, if present, were wholly subconscious. As the standard increases in weight, the marginal sensations loom up in consciousness more and more, and gradually the muscular sensations gain the ascendency. The weight of the arm gradually insinuates itself into the standard. With their cumulative influx, up to certain limits, the auxiliary marginal sensations probably increase the acuteness of discrimination. When very great, however, they may become sources of distraction, and then discriminative sensibility diminishes. It is probably impossible to isolate completely these various sources and bases of judgment so as to determine the comparative worth of each

for various standards, and within what limits Weber's, Fechner's or any other law holds. It may be that some law holds for each of the several bases after it becomes a factor in the discrimination, probably Weber's, but that no law has been or possibly can be formulated to apply to the sum of them.

In these three studies we have important contributions to the detail and theory of motor and allied judgments, and they cannot fail to stimulate further inquiry into these difficult problems.

E. B. DELABARRE.

BROWN UNIVERSITY.

SENSE OF TOUCH.

Die Raumschwelle bei Simultanreizung. ARTHUR BRÜCKNER. Zeitschrift für Psychologie und Physiologie der Sinnesorgane, Bd. 26, Hefte 1 u. 2, May 24, 1901. Pp. 33-60.

This paper reports a study of the conditions under which the fusion of touch sensations takes place. All the experiments seem to have been performed with very great care. Dr. Brückner constructed an æsthesiometer which enabled him to give absolutely simultaneous contacts and to regulate the force of the impacts. Most of the experiments were made with the points from 2 to 30 mm. apart. He found that when two points seem like one the sensation produced by the double stimulus is stronger than that produced by a single stimulus, and that two impacts, each so faint as to be imperceptible in itself, may be perceived when given together. This he explains physiologically, as due to summation. The percentage of cases of apparent summation at the various distances within 30 mm. is nearly the same. The average is 81.5 per cent. In order to bring out the summation results he finds that the subject must give his attention wholly to the strength of the stimulus and not interest himself in the perception of two points.

The most interesting part of his article, perhaps, is that in which he develops a new method of obtaining a threshold. Instead of seeking the distance at which two points can be distinguished, he suggests finding how far apart two stimulations, either of which by itself would be imperceptible, may be and still fuse into one perceptible sensation. However, various factors, chiefly psychic, affect the judgments so that the distance does not remain constant. The double stimulus when perceived as one is often located at a point different from either of the two points actually touched, and when the distance of the two points is relatively great it is often located between them.

J. F. MESSENGER.

COLUMBIA UNIVERSITY.

DISEASES OF ORIENTATION.

Les maladies de l'orientation et de équilibre. J. GRASSET. (Bibl. scient. intern.) Paris, F. Alcan. 1901. Pp. 291.

This exhaustive study of the disorders of orientation and equilibrium consists of material gathered for a series of lectures recently delivered at the Université de Montpellier.

The discussion is divided into six parts. In Part I. a number of typical cases are described, with symptoms covering the various manifestations of these disorders; all the cases chosen were within the author's personal observation, most of them being actually demonstrated in connection with the lectures. Part II. is a survey of the nerve courses and centers concerned in the functions under discussion. Part III. is a brief classification of the disorders of equilibration, a term which the author uses to embrace the two functions of equilibrium and orientation. In Part IV., which comprises nearly two-thirds of the work, the symptoms are presented in systematic detail, as a result of which the seat of disorder in the nervous system is worked out for the several cases and embodied in a schematic diagram in Part V. Part VI. is a brief discussion of the physiological treatment of the disorders in question.

A large portion of the material presented in this book, and that which is more especially of interest to the psychologist, has already been published by the author in an article in the *Revue Philosophique* for March and April, 1901, a résumé of which was given in this REVIEW for September (p. 529). While this has been very much extended in places, the additions are generally for the benefit of the clinical student, and the discussion in the *Revue Philosophique* is probably better suited to the general reader. The amplified schemes, diagrams and tables which are given in the book, however, deserve the attention of those interested in the physiology and pathology of these functions.

H. C. W.

EPISTEMOLOGY AND ETHICS.

Essai critique sur le droit d'affirmer. ALBERT LECLÈRE. Paris, Félix Alcan. 1901. Pp. 263.

In this essay the author has attempted a rehabilitation of the teachings of Parmenides and the introduction of the old Eleatic spirit into the modern controversies concerning the problems of ontology and epistemology. He maintains, quite dogmatically it would seem, that the sole source of certitude is in the region of pure thought, or 'thought in itself,' which lies above and distinct from the lower and uncertain

level of empirical consciousness. The real, accordingly, is that which evidences itself through the sheer force of self-affirmation. The unreal lacks this self-assertive coefficient. We come, therefore, to deny reality to every concept which appears under contradictory aspects. The fundamental law of thought is the law of identity; it, moreover, is perfectly and satisfactorily comprehensive. From this fundamental postulate the author deduces the unreality of the phenomenal world, and, inasmuch as science has to do exclusively with the phenomenal, it must, therefore, be a science of the unreal. In other words, the phenomenal world necessarily resists all attempts at unification. There can never be a metaphysic of science. The phenomenal must be received merely at its face value. A further interpretation, a deeper significance, a final generalization are alike impossible.

The real, therefore, is that which lives, moves and has its being in the realm of pure thought. The essence of reality is personality and the essence of personality is freedom. Hence, it is possible to deduce the being of God, and God thus conceived becomes the norm of reality, and the supreme object of religion, which is the necessary complement of reality. Such, in the main, is the rough outline of the system. The author's point of view, it seems to me, presents the evident limitations of a too refined abstraction; his thought, therefore, lacks that wealth of content which only the concrete can give. There are two ways of rising to the higher level of pure thought, one by leaving the lower level absolutely and thereby denying its reality altogether, the other by so sublimating the lower that it preserves and manifests its reality in the higher. The author, it seems to me, labors under the disadvantage of pursuing the former of these two methods rather than the latter.

JOHN GRIER HIBBEN.

PRINCETON UNIVERSITY.

The Utilitarian Estimate of Knowledge. PROFESSOR JAMES SETH. The Philosophical Review, July, 1901, pp. 342–358.

This article is of special interest at the present time in the light of the recent publication of Mr. Leslie Stephen's 'Utilitarianism.' The difference in point of view is of course a radical one. Professor Seth's insistence upon the value of knowledge for its own sake, irrespective of its utility, is a most timely service. His conclusions may well be taken as an antidote to the doctrines of Mr. Stephen, as expressed in his 'Utilitarianism'; at least, they fulfil the function of emphasizing the fact that there may be another side to the question, and that it has a case which merits at least a considerate hearing.

Professor Seth's position is briefly as follows:

There is such a thing as knowledge which has no instrumental value, and yet carries with it a worth of its own. It is to be assessed not by any external standard, but by one which lies within. Much of knowledge is for the will, but not all knowledge. If it is regarded as having merely a practical value, it tends to lose even that value. There is here a paradox which is similar to that of hedonism. Moreover, the reflex influence of the disinterested pursuit of truth upon the scholar himself has an ethical value which can not be too highly estimated. The discussion is, however, generally epistemological rather than ethical.

The article abounds in historical allusions to the various systems of philosophical thought which have failed wholly or in part to allow a non-utilitarian factor in knowledge, notably the undue estimate on the part of Kant of the importance of the practical reason.

This plea for pure science is one which merits very grave consideration, whatever may be one's particular views concerning the general doctrine of utilitarianism. The tendency of the present day thought is to emphasize unduly the art of knowledge and to overlook completely, in certain quarters at least, the science of knowledge. This fact has a marked pedagogical significance. The average student to-day in our universities, if not the whole student body, determines his choice of studies in a large measure by their supposed utility in reference to the work or profession of life; and interest in intellectual pursuits rises and falls with barometric exactness according as there may or may not be evidence of the possibility of some practical application. This is but one of the many indications at the present time, that these exclusively utilitarian ideas are in the air, and have become all-pervasive. It seems to me that the essential characteristic of a scholar is his devotion to some form of pure knowledge for its own sake and that as there is a categorical imperative in ethics, so likewise there is a categorical imperative in scholarship. And in so far as the scholar comes to pursue knowledge with an eye askance as to the benefits which may accrue either to himself or the world at large, he is so far forth less worthy of the name of scholar.

JOHN GRIER HIBBEN.

THE EMOTIONS.

Les Timides et la Timidité. PAUL HARTENBERG. Paris, Alcan. 1901. Pp. xv + 264.

Those who have any regard for the 'old' psychology are sufficiently warned in the preface that this book is not for them. It is a monograph in scientific psychology; and this is neither more nor less than

a study of brain-functions. On the other hand, those physiologists who have taken endless pains to find out something about cerebral functions, must confess that Dr. Hartenberg's method has the charm of novelty. With the exception of one plethysmograph tracing, there is no evidence in the book of experimental work. In fact, the author says it would not have given us any new information. This may be true; it may even have occurred to those antiquated psychologists who imagined that there was more for them to study than the functions of the brain. But they would not have made amends for the lack of experiment by such lengthy and well-chosen extracts from Amiel, Stendthal, Rousseau and Bourget as are offered in these pages. The value of these extracts from various departments of literature lies in the fact that they show the results of introspection and careful analysis. The author deserves credit for having arranged them under suitable heads and using them to illustrate his definition of timidity and his description of its symptoms.

Timidity is a combination of fear and shame—both groundless—which is felt in the presence of other persons. Its symptoms are, on the organic side, trembling, blushing, disturbances in speech and in the visceral and secretory functions. These are accompanied, on the psychical side, by derangements in the processes of attention, reflection, volition and memory.

The most satisfactory chapter in the book is the third, entitled 'Le Caractère des Timides,' or, on some pages, 'Caractère des Timides.' A distinction is made between the primary qualities of timidity and the modifications which it produces. The former include sensitiveness, fear of ridicule, scrupulosity and a certain secretiveness which is due, not to reserve, but to the dread of being misunderstood. The resulting modifications show a peculiar blending of opposite traits—misanthropy and benevolence, humility and pride. Further consequences are egotism and dilettantism, repression of the feelings and, eventually, inability to express them. In some cases, this self-restraint is followed by an explosive reaction, a sudden display of courage and energy which, for the time being, are without bounds. Either the primary qualities or the secondary modifications may be exaggerated or complicated to such a degree that the health of the patient is impaired—pathological forms of timidity result. These as well as the milder types may, like other diseases, yield to treatment. The patient himself should be urged to cultivate sociability; and various means are suggested by the author. These, in morbid conditions, are to be supplemented by medical treatment.

CATHOLIC UNIVERSITY, WASHINGTON, D. C. E. A. PACE.

HYPNOTISM.

Somnambulism, Hypnotism, Suggestion and Kindred Questions before the Fourth International Congress of Psychology. Compte Rendu des Séances.

To these problems the third general session of the Congress and the three sessions of Section V were entirely devoted. Many of the papers presented are given by title only or in abstract in the *Proceedings* and must therefore be left out of consideration for the present. Many of the others hardly deserve better treatment. None of those whose subject matters fall within the pale of orthodoxy present either facts or theories of any considerable significance. Even Professor Flournoy's contribution to the history of Mlle. Hélène Smith goes but little beyond what he had already told us in his monograph.

The 'kindred phenomena'—which seems to be a screen for the 'psychic research' bogey—fared but little better at the hands of their would-be champions. Three papers of real interest were read, all dealing with Mrs. Thompson, an English automatist, seemingly of the type represented by Mrs. Piper. In one Mr. F. W. H. Myers sums up the phenomena he has observed in her case and the conclusions to which he has been led. In a second Dr. Van Eeden gives some facts, discusses the alternative theories, and concludes by admitting (p. 130), "I cannot doubt that for some minutes I had to do with the voluntary manifestations of a deceased human being." But this conviction attaches in his mind to but a very small part of the phenomena. A large part of the remainder he ascribes to the histrionic gifts of Mrs. Thompson's subliminal self. The third paper, by Mrs. Verrall, is more interesting because it gives more facts, but she does not attempt to base any theory upon them.

Alfred Grafé describes at length some experiments upon a supposed clairvoyant. But the descriptions of the conditions under which they were performed is not detailed enough to enable one to judge whether the possibility of fraud was excluded.

Dr. John E. Purdon briefly reports cases observed by him in 1881 and 1882, in which the pulse-wave of one patient seemed to be telepathically transferred to another, the patients giving identical tracings. It is to be regretted that no more careful and detailed record was made—or published—of these cases.

None of the remaining articles call for special mention. Some were utterly unworthy of the occasion. "Ce sont," says one speaker, "des impressions littéraires, des confessions, de certaines professions

de foi, mêlées avec une regrettable ignorance des documents scientifiques, enregistrés dans la psychologie depuis des années" (p. 617). It is to be regretted that the tolerant spirit displayed by the organizers of the Congress in granting a hearing to the representatives of views with which few of them had any sympathy should have been in some cases so ill rewarded.

W. R. NEWBOLD.

UNIVERSITY OF PENNSYLVANIA.

NEW BOOKS.

The World and the Individual. JOSIAH ROYCE. New York and London, The Macmillan Company. 1901. Pp. xx + 480. $2.25.

Inductive Sociology. FRANKLIN HENRY GIDDINGS. New York and London, The Macmillan Company. 1901. Pp. xviii + 302.

The Evolution of Sex. PATRICK GEDDES, J. ARTHUR THOMSON. London, Walter Scott. 1901. Pp. xx + 342. $1.50.

The Limits of Evolution. G. H. HOWISON. New York and London, The Macmillan Company. 1901. Pp. xxxv + 396.

The Adversaries of the Sceptic. ALFRED HODDER. London, Swan Sonnenschein & Co., Ltd.; New York, The Macmillan Co. 1901. Pp. 339.

G. T. Fechner. W. WUNDT. Leipzig. 1901. Pp. 92.

NOTES.

THE American Psychological Association has fixed the first day of its Chicago meeting for December 31st. The Western Philosophical Association will this year meet in conjunction with the American Psychological Association.

THE next International Congress of Physiologists will be held at Brussels in 1904, under the presidency of Professor Heger.

THE Hon. Oscar Straus has given to the University of Georgia several hundred dollars toward an equipment for work in experimental psychology.

The lectureship in connection with the California Philosophical Union for the current year has been offered to, and accepted by, Professor R. M. Wenley, of the University of Michigan.

Clark Wissler, A.B. (Indiana), Ph.D. (Columbia), has been appointed instructor in psychology in the School of Pedagogy of New York University, and Dr. J. E. Lough has been promoted from an instructorship to an acting professorship.

Dr. Adolph Meyer, director of the clinical work and laboratory of the Worcester Insane Asylum and docent in psychiatry in Clark University, has been selected as director of the Pathological Institute of the New York State Hospitals.

The sum of about $12,000 has been subscribed for the memorial in honor of the late Professor Henry Sidgwick. It will be used for the establishment of a lectureship in moral science in Cambridge University.

Dr. Arthur König, associate professor of the physiology of the sense organs at the University of Berlin, and director of the physical section of the Physiological Laboratory, died on October 26th, at the age of forty-five years. Dr. König was an assistant of Helmholtz's and aided in the preparation of the second edition of the 'Physiologische Optik.' He had carried out important researches on vision, and, with Professor Herm. Ebbinghaus, edited the 'Zeitschrift für Psychologie und Physiologie der Sinnesorgane.'

A. A. Tokarsky, head of the Moscow Psychological Laboratory, died on July 22d.

Dr. Alexander Hughes Bennett, known for his work on diseases of the nervous system, died in London on November 1st, at the age of fifty-three years.

R. Koenig, of Paris, well-known for his researches on acoustics and for his acoustical instruments, has died at the age of sixty-nine years.

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THE PSYCHOLOGICAL REVIEW.

RECENT LOGICAL INQUIRIES AND THEIR PSYCHOLOGICAL BEARINGS.[1]

BY PROFESSOR JOSIAH ROYCE,

Harvard University.

The American Psychological Association has always given a kindly recognition to the general philosophical interests which many of its members represent, as well as to the more distinctively psychological concerns which properly form the center and the main body of its undertakings. In honoring me, by calling me to fill for the year the office of president, my fellow members have well known that they ran the risk of hearing a discussion rather of some philosophical problem than of a distinctively experimental topic. I, in my turn, am quite unwilling to ignore or to neglect the fact that ours is primarily a psychological association, while I am equally aware that the general student of philosophy is at a disadvantage when he tries to discuss with the productive workers in the laboratories the matters which, as their specialty grows, come to be increasingly their own peculiar possession. Yet a presidential address is properly an opportunity for studying the problems suggested by a comparison of various fields and methods of work. And accordingly, upon this occasion, I propose to discuss some questions that lie on the borderland between psychology and the distinctively philosophical disciplines. These questions in part directly touch undertakings which already occupy a recognized place in the psychological laboratories. In part they seem to me to

[1] Address of the president before the American Psychological Association, Chicago meeting, January, 1902.

promise to yield in future still wider opportunities for experimental research than are now open. In any case they are questions of permanent interest, and of increasing importance, which neither the psychologist nor the philosopher can afford to ignore.

I.

I have named my paper a discourse upon 'recent logical inquiries and their psychological bearings.' By the term 'recent logical inquiries,' I mean to refer to two decidedly distinct classes of researches, both of which are to-day receiving much attention. To the first of these two classes belong researches directly bearing upon the psychology of the thinking process, and upon the natural history of logical phenomena in general. Such inquiries may be called logical, since they are sometimes undertaken by logicians for the sake of their own science, and in any case are suggested by the problems of logic. Meanwhile, studies of this class are obviously also, at least in intention, contributions to psychology. But I wish, in addition, before I am done, to call attention to quite another class of researches, whose psychological bearing is not at first sight so evident. This my second class of recent logical inquiries consists of studies in the comparative logic of the various sciences, and of examinations of the first principles of certain special sciences. I refer here especially to such books as Mach's well-known volume on the 'Principles of Mechanics,' and to all the large literature that has grown up about the problems suggested by the fundamental concepts of the different natural sciences. I place in the same class, moreover, the elaborate and fruitful researches into the foundations of arithmetic, of geometry, and of the Theory of Functions, which are due to such mathematicians as Cantor, Dedekind, Peano, Klein, Hilbert. The last three or four decades have seen an enormous extension of the literature of this type. I include, moreover, in the same class, certain more distinctively philosophical treatises such as Russell's 'Essay on the Foundations of Geometry,' and Couturat's volume on the 'Concept of the Infinite,' and these are but specimens of the class of inquiries in question.

I mention this vast collection of significant studies, not be-

cause I am in any sense a master in this field of the comparative logic of the sciences, but because, as a humble learner, I have been trying to make my way in some of the plainer of the paths that these recent studies have been opening, and because I hope, by a few wholly inadequate, but at least timely indications, to show upon this occasion that this relatively new comparative study of the fundamental conceptions of various sciences, is full of promise for the psychologist as well as for the logician.

Of the intrinsic importance of this my second class of 'logical inquiries,' there can be, in many cases, no doubt. From the literature of comparative logic to which I thus refer, there is certain to grow, with time, a new science, which I may venture to call a comparative morphology of concepts. This science will occupy a borderland position. In one respect, it will belong to philosophy properly so called. For it will lead to advances in just that critical consideration of the foundations of knowledge which constitutes one principal division of philosophy. Upon the other hand, the new science will be an empirical as well as a reflective doctrine. It will include a critical examination of the history and evolution of the special sciences. And in this respect it will take its place as a contribution to the general history of culture, and will furnish material for the student of anthropology and of social psychology. And, still further, the new science will contribute to the interests of the student both of general and of experimental psychology. For it will set in a new light the empirical problems of the psychology of the intellect. It will define, in new form, issues which the descriptive psychologist must attempt to reconsider. And, as I am convinced, it will present an ample array of problems for the experimental psychologist,—problems which he alone will be able to pursue into some of their deepest recesses. This new science, then, which you and I can hardly live to see very highly organized, but which the whole century now beginning will greatly advance, will offer large ranges of what one may call neutral ground, where philosopher and psychologist, special student and general inquirer, historian and sociologist, may seek each his own, while a certain truce of God may reign there re-

garding those boundary feuds which these various types of students are prone to keep alive, whenever they discuss with one another the limits of their various territories, and the relative importance of their different tasks.

II.

Two distinct and very large classes of 'logical inquiries' my title is thus intended to bring at once to your attention. My reason for naming them by means of one phrase, and for considering them in one paper, is this: When you examine the first of my two classes of recent inquiries, you find that while much is now doing to advance our knowledge of the psychology of the thinking process, we have to admit that the present state of research in this field is not wholly satisfactory. The general theories about what the place of thought is in the natural history of our minds, and about the special processes of which thinking consists, are numerous; but regarded as psychological theories, they still seem for the most part loose and ill-founded. On the other hand, the special efforts to break paths into the thickets of the psychology of the thinking process by means of experimental research, have so far met with serious obstacles, have often given negative results, and in any case have been confined to the outskirts of the subject. A survey of our first class of recent inquiries will therefore suggest to us the need of looking in new directions for additional sources of aid in the study of the psychology of the higher intellectual processes. In view of this fact it may appear, before we are done, that there is a genuine promise of help towards further advances in this branch of psychology, in case we look for such help to what I have called my second class of recent inquiries in logic. These studies in the comparative logic of the sciences are at once, as I have said, philosophical and empirical studies. They are logical researches regarding the foundations of knowledge. They are also historical reports regarding the way in which our human thinking processes have worked and are working in the world of live thinkers and of socially guided investigations. To call attention, in however feeble or summary a way, to the evidence that is thus attainable regarding the natural history of the think-

ing process, is a purpose that may justify my necessarily very superficial comments upon this branch of my topic.

III.

And so let me next say something about the first of my two classes of recent inquiries, namely, those that are more obviously and explicitly guided by psychological motives.

The psychology of the intellect is one of the oldest branches of psychological inquiry. In Greece it began in pre-Socratic philosophy. It became prominent in Aristotle's doctrine. Both Stoics and Epicureans contributed to it. Scholasticism elaborated and modified Aristotle's theories regarding the whole province. Modern philosophy, and in particular the English psychology, began with renewed interest in the problems of this branch of mind. Thus, the psychology of knowledge was long the favored child of the philosophers, at times when the feelings and the more purely volitional aspects of mental life were comparatively neglected in their researches. In a sense this advantage of the intellectual process has continued in recent times. The psychology of association and that of perception have been steadily advancing. Attention, discrimination, and lately memory, have been experimentally studied. But on the other hand, in recent psychology, just the region where, at the outset, the interest of the philosophers was early centered, namely, the region of study of the higher intellectual processes—conception, judgment and reasoning—is the very province of psychology where progress, in any exact sense, is nowadays so slow. The difficulty of reducing the problems which, for the psychologist, arise in this region, to any form capable of exact experimental inquiry, is notoriously great, and will of course long remain so. Meanwhile, however, the actual importance which psychological methods have won in the esteem of modern writers, have led to repeated attempts to found reforms in logic upon psychological theories. Numerous are the modern works on logic wherein the psychology of the thinking process is expounded at the beginning of the whole research, or at least is made the basis upon which an author's logical doctrines depend. The great influence of Brentano's doctrine of the process of

judgment upon one whole series of logical inquiries in Germany is well known, and is an example of what I mean. The earnestness with which the problem of the nature of the 'impersonal' judgments has been discussed by a large number of modern writers on logic is another example of this subordination of logical to psychological issues. For the doctrine of the 'impersonal' forms of expression is a problem of the psychology of language, and to my mind, interests the pure logician hardly at all.

Meanwhile, if psychological doctrines have thus played a large part in the books upon logic, one can hardly feel surprised to find that, in the present state of the psychology of the intellect, the theories about the higher intellectual processes which have been expounded in the logical treatises have been somewhat dishearteningly various and capricious. Concerning the processes of abstraction and conception, certain stereotyped formulas were indeed, until quite recently, pretty constantly repeated. But with the doctrine of judgment, chaos in the textbooks of logic began. Judgment was, so one sometimes said, a process of pure association of ideas, wherein the subject idea recalled to mind by contiguity the predicate idea. But no, said others, it was rather a process of Herbartian apperception, wherein the predicate idea assimilated the subject idea and forced it to fuse with itself so that they became but one idea. On the other hand it was often something much nobler; it was an active process of synthesis, not to be confounded either with mere association or with passive fusion—a constructive process wherein subject and predicate idea came to be connected by certain peculiar mental links. Yet not so; on the contrary, it was a process of analysis, whereby a given whole was divided into parts, and the subject and predicate were the products of this sundering. Or, yet again, it was no union and no sundering of ideas at all, but something quite different—an estimate about the objective value of a connection of ideas. But still once more, it was none of all these things, it was an entirely irreducible act of accepting or rejecting an idea or a complex of ideas; and upon this psychologically irreducible and primal act was founded our very conception of any distinction whatever

between the objective and the subjective world. All these things judgment has been in the text-books, and this, as you well know, is not the end. And all these views have been advanced, upon occasion, as psychological theories about the process of judgment, as theories either verifiable by direct introspection or else deducible from more general doctrines about our mental processes.

In presence of such a variety of opinions, many students interested in the theory of the thinking process have tended, in more recent discussion, to choose one of two opposed directions. Either they have been disposed to relieve themselves altogether of any responsibility for settling the psychological problems, by drawing a technically sharp line between Logic and Psychology, by devoting themselves to the former, and by leaving out of the logical inquiry all consideration whatever of the descriptive psychology of thinking; or else, choosing rather the psychological road, they have attempted to reduce the problems in question to some shape such as would make possible a more exact introspection of the details of the thinking process by causing these to occur under experimental conditions. The former of these two ways of dealing with the problem of the nature of the thinking process has recently been formally adopted, amongst other writers, by Husserl, in his *Logische Untersuchungen.* Husserl has vigorously protested against all *psychologisirende Logik.* Logic, he insists, must go its own way, yet Husserl, in his still unfinished and very attractive researches, yet lingers over the problems of what he now calls the 'phenomenological analysis' of the thinking process, and his farewell, as a logician, to psychology proves to be a very long one, wherein the parting is such sweet sorrow that the logician's escape from the presence of psychology is sure to lead to further psychological complications. As a fact, I cordially accept, for myself, the view that the central problems of the logician and of the psychologist are quite distinct, and that the logician is not responsible for, or logically dependent upon a psychological theory of the thinking process. Yet I am unable to doubt that every advance upon one of these two sides of the study of the intellectual life makes possible, under the conditions to which all our

human progress is naturally subject, a new advance upon the other side. I believe in not confounding the tasks of these two types of inquiry. But I do believe that a mutual understanding between the workers will be of great importance; and I feel that we need not discuss at very great length, or insist with exaggerated strenuousness upon the mere separation of provinces in a world of inquiry wherein to-day there are rather too many sunderings.

Meanwhile, as to the other way of approaching the problem of the nature of the thinking processes, namely the way of attacking them from the side of a more careful application of the methods of recent psychology, that at present, as I have said, is beset with well-known difficulties—difficulties upon which I need not dwell long in this presence. The most important thinking processes do not occur under conditions such as either the subject in the laboratory can easily reinstate at will, or the experimenter can determine for the subject while the latter is under observation. The thinking processes upon which experimenters have so far carefully worked are therefore artificially simplified ones—important, but elementary. The numerous investigations regarding the process of the perception of small differences of various types belong here, and constitute, in one aspect, a contribution to the psychology of judgment. The mental reactions upon the presentation of words and phrases, heard or seen by the subject, have been studied by Ribot and by others. Recently Marbe has undertaken to investigate experimentally the psychology of judgment, although under conditions that I have to think by no means very satisfactory. Simple computations, acts of recognition, of estimate, of naming, have also been investigated in various laboratories. But as you know, the positive and assured results of such work have been by no means all that one could wish. Especially notable has been the decidedly negative result of a good deal of this investigation of artificially simplified thinking processes. While, to be sure, the study of the perception of small differences has shown how unexpectedly complex are the psychophysical conditions upon which such judgments depend, the effort in case of even much more complex and intelligent thinking processes to

find present in consciousness contents as complex as those of a rational thinker ought to be, has not met, under experimental conditions, with the success that one might have hoped for.

Ribot discovered that in many cases, when one presented to the thoughtful subject a general term whose meaning was somewhat abstract, but nevertheless familiar to him, and when one asked him what mental contents the suddenly presented term directly brought to mind, the answer was simply, 'nothing.' Marbe, dealing with trained subjects, of scientific habits of mind, made them perform and express simple acts of judgment, under experimental conditions, and asked them to observe introspectively the conscious accompaniments of these acts. He found, in general, that the subjective accompaniment of the judgment, apart from the direct consciousness of the very act whereby one gave expression to the judgment, was nothing at all characteristic, and was very often, as in Ribot's subjects, simply nothing at all. The subject in Marbe's experiments was to make a judgment of some intellectual value, but pretty easily accessible to him, regarding a certain presented content; as, for example, he was to choose which one of the two perceived objects had a given character; or he was to answer some other simple question, regarding facts or ideas presented to his attention by the experimenter. He was at once to express this judgment, by word, or by other motor process, as the case might be. He was then to report what mental accompaniments the act of judging had involved at the critical moment. The result of the experiments was to show that these well-trained thinkers responded to the situation in question in a mainly reflex fashion. They expressed their discriminations, their translations of Latin phrases, or their other simple intellectual processes, with relatively little difficulty; and all that was characteristic of the conscious process at the moment was that they observed, of course, the expressive act itself, which they chose in a conscious sense no more and no less than one chooses any other complicated reflex act of high grade such as comes to consciousness while it is carried out. For the rest, they sometimes observed fleeting states such as doubt or surprise, and various chance associated images, or suggested motor

sensations, of no importance for the understanding of what it is to judge. These accompaniments of the act of judgment were merely individual accidents.

Such negative results have appeared, upon second thought, not very surprising either to Ribot or to Marbe. Ribot points out that most of the connected and significant processes of our life have to be largely unconscious, just because we are conscious only from instant to instant, while we live with reference to relatively far-off results, and while the rational connections of life have to do with long periods of time. The organization of our intelligent conduct is necessarily, he thinks, a matter of habit, not of instantaneous insight. And a complex abstract idea, as Ribot points out, is a 'habit in the intellectual order.' "We learn to understand a concept as we learn to walk, dance, fence, or play a musical instrument. * * * General terms cover an organized latent knowledge which is the hidden capital without which we should be in a state of bankruptcy." Marbe comforts himself for his negative results with the reflection that a '*Wissen*' can never be, as a content, itself '*im Bewusstsein*.' The subject judging knows, as Marbe maintains, what the act means, but no conscious content directly corresponds to or embodies this knowledge. The only necessary conscious content that is present to the subject corresponds to the outward act, the speech or gesture, whereby the subject expresses his meaning, and this, in Marbe's opinion, sufficiently explains the negative result of his own experiments.

No doubt these comments of Marbe and of Ribot have a good deal of justification so far as concerns their own experiments. On the other hand, however, we cannot feel that their experiments were at all well adapted for observing the wealth of our actual thinking processes, because what they studied was not, in most cases, any process by which a thought can come to be built up in our consciousness at all. They could not thus hope to decide how far thought ever can find a peculiar or characteristic place in human consciousness. For what they both examined were relatively reflex processes that express the mere residuum of a mental skill long since acquired by their subjects. Ribot himself thought, and no doubt consciously

thought, when he *planned* his experiments; Marbe thought, when he considered what problem to choose for presentation to his subjects. But the subject (already, in the mentioned cases, a person of relatively high training), had little or no need to think at all in a situation as simple or as familiar in its type as the one in which the experiment placed him. Therefore it was the experimenter and not the subject in whom the process that was to be studied went on. The subject already long since knew how to meet the familiar abstract term, or to translate the simple phrase, or to answer the other plain question. Either this his previous training disposed him to wait passively, upon hearing the well-known word, until he should have some reason to use it himself, or to bring it into connection with his own acts; or else just such training had prepared him (in Marbe's experiments) to accomplish the act whereby one could express a judgment upon the simple problem presented, or could otherwise easily and instantaneously show one's accustomed skill. In no such case was it necessary that any notable intellectual contents of higher grade should come to the subject's consciousness. The mechanism established by long training was ready. It responded as the training determined. Consciousness showed indeed nothing of an abstract thinking process; but then there was no live thought present to show. Ask me "What is the sum of 3 and 2?" or "Who was Washington?" and very probably I shall just then not think at all. If I am disposed, under experimental conditions, to respond to your question, without knowing beforehand what the question is to be, I shall, upon hearing such an inquiry, respond as smoothly as if I were a wholly reflex mechanism. And very naturally I shall then have nothing to report in the way of introspective facts of a thoughtful sort. For I shall respond much as a baggage clerk at a large station calls out the names of cheques, or as a telegraph operator writes out his messages while listening to the familiar clicks of the instrument.

To say this is not to make light of experimental methods in their application to the psychology of thought, but is to show that if the problems of the psychology of the intellect are to be prepared for more effective and advanced experimental research

in future, the thinking process must first, in some measure, be more fruitfully analyzed than has yet been the case, into elementary processes of a type capable of separate experimental study. On the other hand, the way in which these processes are synthesized into the richer life of concrete thinking must be discovered mainly in an indirect fashion, through an examination of the expressions of thought in the various products of the human intellect, as they appear in language, in social institutions, in the mechanical inventions and constructions which human reason has made, and in the constitution of the sciences themselves—those highest expressions of man's ingenuity. Meanwhile, as I think, a preliminary examination of these very larger expressions of the intellect themselves, may also help us to proceed further than we have yet done in the preparatory analysis of the elementary activities upon which our thought depends, and may enable us thus to open the way towards such an experimental investigation of the conscious aspects of live thinking as just now we lack.

What then is the best means to make such a preliminary analysis of the thinking process into its elements? To analyze thought by means of a study of the phenomena of language has so far been, from Plato's time onwards, the principal undertaking of those who have approached the psychological problems of the intellect from the objective side, that is, from the side of the way in which human thought has outwardly expressed itself. The logicians and the psychologists have joined in a frequent examination of the phenomena of speech. Both types of investigators have sought thus to acquire a knowledge of what the thinking process essentially is. And this sort of inquiry still prospers. A recent logician, Benno Erdmann, has undertaken elaborate studies in this field, studies that have combined the analysis of pathological facts with those experimental researches which he and Professor Dodge have made so well known. From the psychological side, and with vast resources in the way of varied materials, Wundt has also lately prepared his really wonderful volumes on language, working with all the equipment of the experimenter, the logician, and the philosopher, but carefully distinguishing the task of this recent book from that of his own

earlier treatise on logic. One may say, then, that the psychology of language is indeed in a progressive state. Yet I cannot but hold that the relation of language to the thinking process has been somewhat too exclusively emphasized by many students of the subject. Thought has other modes of expression than through the forms of speech. Language has other business besides the expression of thought. Wundt's book has the merit of emphasizing the close and primary relation of language to the expression of the feelings and to the life of the will. In consequence, Wundt very decidedly sets limits to the tendency either to regard the grammatical categories as essentially logical ones, or to use the psychology of language too exclusively as a means for interpreting the psychology of the thinking process. For this very reason his book rather encourages one to look elsewhere for auxiliaries in comprehending the psychology of the intellectual life.

I have thus endeavored to sketch some of the more directly psychological of the recent inquiries into the nature of the thinking process, in order to show why, despite all these various developments, I myself think that the psychologist still has much to learn from researches in other fields than those in which he has so far been most accustomed to seek for help. These other fields are the very ones which are opened by those recent inquiries in the comparative logic of the sciences of which I spoke at the outset.

IV.

Some widespread influence, it is hard to tell exactly what, has led, during the last three or four decades, to repeated, and often seemingly independent and spontaneous, efforts on the part of the students of various special sciences to undertake an examination into the first principles of their own branches of inquiry. The mathematicians say that it was the discovery of errors in certain accepted theorems or proofs of theorems which was the principal motive leading to their own modern desire for an increased rigidity of methods, and an increased clearness regarding their fundamental assumptions. A wide extension of some of their earlier conceptions, such as the conception of a function, resulted, during the nineteenth century, from the nat-

ural advances of their science. It was found that as such conceptions extended their range of application, theorems to which no exceptions had been known at earlier stages of the science became obviously of restricted application in the new fields thus opened, and often had to be restated altogether. In consequence, proofs of these theorems which had been accepted as valid in earlier stages of the science, were seen, in the light of the enlarged conceptions, to be invalid, or to be capable of rigid statement only through the addition of precise qualifications which had earlier escaped notice. Thus there arose a keenly critical consciousness about what constituted exact statement and rigid proof. Moreover, mathematicians are especially disposed by the work of their science to compare together the results of various and apparently independent sorts of inquiry. Especially is this the case when one considers the relations of geometrical and analytical science. At one time geometrical intuition, at another time analytical computation, may lead in the advancement of mathematical knowledge. The question therefore constantly arises, Which of these two sorts of inquiry is the superior in power, or in logical exactness? Such comparisons must lead to constantly renewed self-criticism passed by the science upon itself.

Again, early in the nineteenth century, the constructive imagination of certain geometers of genius initiated an examination of the foundations of Euclidean geometry which has since proved of the utmost importance as a study in the fundamental concepts of all science. Such influences long worked in a comparatively isolated way. Towards the close of the century they combined to bring about a sort of common consciousness on the part of mathematicians regarding the methods that they required of the investigator and of the expounder of mathematical truth. This common consciousness expressed itself not only in the regions where the science was advancing to conquer new territory, but in the study of the oldest, the most fundamental, simple and universally human of mathematical ideas. The concept of number is one of the earliest of human scientific acquisitions; yet it has recently been subjected to a searching logical analysis with decidedly novel and unexpected

results, so that nobody can rightly judge what it is to count or to use numbers for purposes of recording measurements, unless he has taken into consideration mathematical discussions that are hardly thirty years old. The various extensions of the number-concept,—the relation between rational and irrational numbers, the relations of number to quantity, the different systems of complex numbers, the conditions logically necessary in order that number systems should be applied to the expression of space-relations,—all these topics have been reviewed from the foundation upwards; and the work still goes on. The various actual or possible conceptions of continuity, the exact meaning to be ascribed to the concepts of numerical and of quantitative infinity, the logical position of the conception of an infinitesimal,—all these matters have been reconsidered with a care and a novelty of result which no one can appreciate who has not come into closer contact with at least a few of these researches. And now what I wish especially to emphasize, is that all these analyses, while their direct purpose is logical, inevitably possess a psychological bearing. For they throw light upon the structure which the universally human processes of counting, measuring, comparing and otherwise dealing with continuous magnitudes have always possessed. They define certain of our most fundamental intellectual interests in our world of experience. They therefore not only logically clarify and in so far transform these interests, but they tend to several otherwise hidden aspects of the natural history of these interests themselves.

For instance, the logical prominence which these modern researches in the logic of arithmetic give to our general concepts of serial order, as contrasted with our more specialized quantitative concepts, involve a generalization about the nature of the thinking process that at once has a psychological application. For we learn hereby to distinguish the activities through which we have formed the conception of any ordered series of facts from the processes whereby we have learned to apply this conception in certain important, but decidedly special, cases to the task of measuring magnitudes. The two processes are different, not only logically, but psychologically. The

second is a highly specialized application of the other, which is the more primitive and the simpler. The new problem that arises for the psychologist is that of the psychology of our ideas of serial order. The forms in which this problem is to be attacked with fruitful success by the psychologist must be furnished to him by the logician of mathematics. The latter discovers by analysis what concepts of order are fundamental and what ones, logically speaking, are derived; and how the more complex forms of order are related to the simpler. The solution of this logical question is of course primarily not any decision of a question of genesis. But it is the answer to the question, What forms of order, what types of serial arrangement are of the most importance in human thinking about the world of experience? This answer inevitably tells us, however, something about what is universal in the actual constitution of those habits of our organism upon which our thoughts about order depend. It is true then that to ask, What is logically fundamental in our ideas of order? is to ask not a psychological, but a logical, question. But to discover what is logically universal, as the basis of our exact ideas, is to find out a process that must be very widely represented in those organized modes of action of which our thoughts are an inner expression. Hence the result of the logician's analysis, while it cannot be directly translated into a logical theory, is inevitably the setting of a definite problem for the experimental psychologist.

As a fact, the problems of the psychology of the concept of order form a field for experimental research whose importance the whole modern logic of mathematics makes daily more obvious, while the adaptability of the problems for the labors of the experimenter is so obvious as hardly to need lengthy illustration. Psychologically speaking, the importance of the order in which facts are presented to us is illustrated by every case of an inverted letter, by every disarrangement of a familiar temporal or spatial sequence, by every instance of the illegibility even of our own handwriting when seen in a mirror. One of our earliest and principal mental interests is in the serial order of things and in the weaving of various serial orders into systems. But mathematical science is in large part an analysis

of ordinal systems. Hence an advance in our analysis of the logical concept of order, and in our knowledge of its range of application, makes possible a more fruitful study of the natural history of thought than would otherwise lie within our power.

In the modern study of the logic of the space-concept, there is again a rich field where the results of the mathematical logicians suggest problems for the psychologist. I have myself been surprised to see how little interest psychologists have generally taken in the space-theories of modern mathematics. There is a remark of Klein, repeated since by a good many writers, to the effect that modern projective geometry, with its non-metrical methods, is rather a description of the properties that are most prominent in visual space, while ordinary geometry, with its quantitative or metrical concepts, is rather founded upon our experience of the space of our touch and of our bodily movements. This remark emphasizes what is indeed an obvious fact. One may pass lightly over it, and think little of it. But its significance begins to dawn when one learns something of those logical relations between non-metrical and metrical geometry which Cayley, and later Klein himself, first made prominent. Projective geometry, taken in the abstract, can be developed without the use of any conceptions whatever of metrical relations in space. In other words, projective geometry is a science of spatial order, and not at all of spatial quantity. Cayley and Klein showed how, by the use of certain (once more, very abstract and ideal) assumptions, our ordinary metrical geometry can be made to appear as a highly specialized case of this purely ordinal science. In the light of this consideration, Klein's just cited remark about the contrast between visual space and tactual motor space suggests a very interesting, although a very complex psychological problem about the psychology of the concepts of order and of quantity in their application to space. I suppose that no psychologist would admit that visual space is primarily non-metrical; and, of course, Klein did not mean that it was purely so. For the rest, visual space is obviously related to our consciousness of the results of our movements, and cannot be isolated from them, except by a deliberate abstraction. But, on the other hand, visual space

certainly does present to us the facts which projective geometry isolates; while our other space experiences do not directly involve these projective facts at all. But the projective facts, as logical analysis shows, are, when taken by themselves, non-metrical, while the laws of the metrical facts regarding space are capable of being conceptually defined as very specialized cases or results, under certain ideal conditions, of the laws of a non-metrical space-world. These considerations may not prove to have important results for the psychology of our concepts of order and of measure; but as they stand, they certainly suggest genuine problems for psychological scrutiny. I wonder, then, to find them so little regarded by the psychological students of the space problem.

In a somewhat different direction various contributions to the questions about our consciousness of space have been made, within the last few years, by M. Poincaré, who has here shown, not only all the knowledge of a great mathematical investigator, but also a decided effort to translate his analyses into psychological terms. These contributions of Poincaré, following the results of Lie and others, have laid stress upon the relation between our general spatial conceptions and the mathematical theory of 'groups'; and they promise in still another way to bring to pass connections between psychological and mathematical investigations. In view of such developments, I feel that the time is approaching when no psychologist will have a right to try to contribute to a knowledge of our space-consciousness, so long as his own geometrical conceptions are still confined to those of the mathematical text-books of his early youth. Psychological space theories must be brought into explicit relation with mathematical theories.

V.

But I must hasten from this mention of the merely mathematical investigations to a still more summary reference to similarly analytic work that has been done in other fields of the logic of science. The books of Mach, whose name I have already mentioned, are surely known to many of you. Dr. Paul Carus has proved, as editor and as director of translations, a bene-

ficent aid to our students in this country by making literature of this type widely accessible amongst us. And you surely know the spirit of much of this modern literature of the logic of science. It is characterized, first, by a certain measure of the same sort of critical skill which has made the modern mathematicians so rigid in their methods of proof, and so critical of their first principles. To be sure, outside of pure mathematics, you seldom meet with the degree of rigidity which that science has of late so carefully cultivated; but still the spirit of watchful self-analysis, the freedom from sacred and unquestionable dogmatic presuppositions of all sorts, the willingness to consider fairly the possibility of the opposite of any once asserted proposition, are the common features which characterize Mach, Pearson, Hertz, and the other typical writers of this recent movement. Even as I have been preparing this discussion there has come into my hands the *Vorlesungen über Naturphilosophie* of Ostwald—a book of whose charm a reading of the first half of the lectures has already convinced me, and whose logical spirit, whatever you may think of its results, is of the most delightful and wholesome. The researches of which such literature is the representative, are characterized by a view of the nature of the thinking process which is closely allied to that which the mathematicians have gradually developed. For one thing, human thought, in the view of such modern writers, is not bound by any one definable collection of unquestionable axioms, nor yet limited in its operation by any mysteriously predetermined set of irreducible primal concepts. It is a variable and progressive process that is concerned with the adjustment of conduct to experience. In place of unquestionable axioms, one has therefore, in any science, only relative first principles, resolutions, so to speak, to treat some portion of the world of experience as describable in certain terms. The immediate purpose of any thinking process in a special science is the description of experience, and is not what used to be meant by the explanation of facts. To describe experience is to construct a conceptual model that corresponds, point for point, so far as desired, with the observed phenomena. In order to construct this conceptual model, one has to set about

one's work with a definite plan of action, a plan large enough and coherent enough to cover the intended range. One's provisionally assumed first principles, or, as such writers often say, one's postulates, are therefore chosen simply, as expressions of this coherent plan of action. One constructs one's model according to these postulates, compares the results with the facts, and is judged accordingly. Meanwhile, a paucity of elementary assumptions is to be preferred, because science, as a practical activity, loves economy. Such writers use the older forms of the principle of causation either not at all, or as sparingly as they think possible,—their reason being that they are not quite sure what the principle of causation used to mean, and that they have a great fondness for entirely overt, sharply definable and clearly verifiable relations amongst phenomena, so that they are interested only in finding such relations. But causal explanations, as formerly conceived, seem to them to have supposed the true connections of facts to be founded in something behind the scenes, which no experience could ever bring to light. Such writers therefore seem to themselves to be working in a purely positive spirit, as August Comte long ago, although in a much cruder fashion, advised us to work. They often, like Mach and Pearson, call themselves anti-metaphysical. Yet, as a fact, all this analysis of the structure of the thinking processes of the special sciences, and of what I have elsewhere called the world of description, seems to me to be not only in no wise inconsistent with an idealistic philosophy, but to be a most fruitful auxiliary to such an idealistic interpretation of the facts of the universe as, in another place, I have had occasion to maintain. But here is no place for considering the philosophical value of such a view of the logic of science. What I am here concerned to show is that this effort so to expound the principles of science as to make all the assumed relations between the objects of one's thought overt and exact, rather than occult and inscrutable, relations, leads of necessity to an analysis of the process of thinking which is full of psychological suggestiveness. For a similar reason, this effort to justify scientific theories solely by their success in producing conceptual constructions that correspond in definite and controllable fashion with

the phenomena, leads to a sort of practical theory of the business of thinking which closely relates the point of view of the logician to that of the psychologist. For the latter must view the thinking process as one of adjustment to the environment; and he must suppose the mental motives which determine the choice of one rather than another way of thinking to be in the long run determined, as to their natural history, by the success of one method of adjustment as compared with that of another.

In consequence, I maintain that the future study of the psychology of the thinking process will have much to gain from a use of such analyses of ideas and processes as this new science of the comparative morphology of concepts will, as it further develops, bring to light.

VI.

My hastily-made catalogue of the types of researches which belong to the second of my two classes of recent logical inquiries is thus, within its present very narrow limits, completed. I must still try briefly, however, to lay stress upon a very important general feature of the thinking process which all these recent researches, whether in the specially mathematical field or in the wider field of the logic of the various natural sciences, seem to have brought to clearer light. So long as logicians were largely confined in their researches to results derived from the analysis of language, the problems which they could hand over to the psychologist were principally the classic, but as I think, relatively fruitless problems, to which Ribot's and Marbe's experimental researches have been devoted—such problems as, What has one in mind on hearing an abstract word pronounced? or, What happens in my mind when I judge that A is B? We have already seen that the modern mathematical researches have prepared for the psychologist a large collection of relatively new problems relating to our consciousness of the types of serial order, and relating also to the way in which this consciousness of order is linked to our ideas of quantity, of space and of continuity in general. Many of these problems have assumed, in modern mathematical researches, decidedly instructive forms, which are now nearly if not quite ready for experimental study. But the problems which modern logical research

is preparing for the psychologist are by no means limited to these. Let me call attention then to another range of problems of a very complex character, but of a type especially likely to receive, I think, ere long, a form suited to novel experimental researches.

Psychologists have already elaborately studied, in the laboratories, our consciousness of the differences between presented objects of various sorts. But a difference between two sensations, or intervals, or other presented facts, is a matter rather of perception than of more elaborate thought. We judge such a difference indeed; but the judgment occurs as a sort of more or less swift or deliberate reflex, subject to no conscious logical principles, except those implied in every least effort to attend to the facts presented, and to report accordingly. Even in such an effort, however, there appears one element that, in the life of our more familiar and complicated thinking, assumes extremely varied and important forms. The subject in a series of experiments upon just observable differences is obliged to report whether two objects appear to him to differ or not to differ in an assigned respect. Upon this side his act of judgment includes what one may call the 'yes' and 'no' consciousness, the decision as between alternatives, the selection or suppression of a certain possible response to an object. But the 'yes' and 'no' consciousness is one that is of course not limited to the case of observing small differences, but that has applications wherever we are able to judge; and one of its most important applications appears whenever we not only observe the differences of objects, but, in some more elaborate way, *classify* objects. Two objects, such for instance as a triangle and a circle, are in two such different classes for us (when we do judge them as figures of different classes), not merely because we observe that they are for us different in shape, but because, in the presence of one of them we are disposed, in view of our geometrical training, and even of our purely popular habits of thought and speech, to make certain responses, to perform certain deeds, which, in the presence of the other object we should, if these deeds were suggested, suppress, reject, inhibit, as unfitting, absurd, untrue. In presence of the circle we do not only tend to follow its con-

tour by means of certain eye movements, and to have suggested to us certain names, memories, and æsthetic impressions; but, if we are thinking about circles we consciously accept certain of our suggested motor responses in presence of the circle, as adapted to express what it means for us, and how it is related to the rest of our life. Some of these very responses to which, in presence of a circle, we thus, so to speak, say 'yes,' are amongst the ones to which, in presence of a triangle, we say 'no,' in case there then arises any suggestion of our making them. Our customary summary expression of the results of many such acceptances and rejections of fitting reactions in the presence of circles and triangles takes the form of saying that 'no circle is a triangle.' This assertion is of course not the same as the assertion that our representative ideas of circle and triangle are different ideas. One's idea of a Frenchman differs from that of a dancing master. But it is absurd to say that because one is a Frenchman he cannot be a dancing master. Our assertion about circle and triangle is that they are not merely different, but belong to mutually exclusive classes. And we define for ourselves this latter fact of the mutual exclusion of the classes by means of a series of processes in which the consciousness of presented or remembered differences is bound up with the 'yes' and 'no' consciousness in a fashion that the logicians and psychologists of all ages have attempted to unravel, and that the psychologists, at least, have failed to discuss with finality, just because they have so little studied the 'yes' and 'no' consciousness, either in itself, or in its relation to our consciousness of difference.

As for the logicians, with their Eulerian diagrams, and their more recent and exact symbolic notations, they have indeed done much to clarify the more formal aspects of the conceptual relations involved in exclusions and negations; but, as Professor Ormond's paper on the place of the negative in logic showed to this association some years since, the questions here involved are amongst the most delicate and fundamental known to thought, and they are not yet closed issues. What, then, is the precise relation of the consciousness of difference to the consciousness of negation, or of mutual exclusion? Both logi-

cians and psychologists need to study this problem more thoroughly.

But now it is just here that the modern reëxamination of the principles of the various sciences has been enlarging our ideas of the importance of the function of what I have called the 'yes' and 'no' consciousness in all our exact thinking. When I first heard about the logic of science, I was told by my teachers that the stage of a science in which it made much of classifications was a relatively imperfect stage. A science, I was told, passed to a higher stage when it learned to substitute explanations for classifications. And its explanations, in their turn, became exact whenever they passed to the highest stage of scientific knowledge, where they became quantitative. Quantity, then, was a concept of a rather mysterious dignity; but it certainly belonged to some very lofty level of thinking, where mere classifications were no longer in question. When one reached this lofty level science became mathematical, and the goal was near.

But nowadays, our new comparative logic of the sciences seems to put this whole matter in a new light. The ideal of exact special science is still mathematical, and will always remain so. But then, for one thing, mathematics, for the enlightened, is no longer merely the science of quantity, but is rather the science of exactly definable relationships of all types. Quantity itself, however, appears, in this new logic, as a conception whose properties and laws, in all the numerous branches of the science of the different kinds of quantity, are definable only in terms of the properties of certain manifolds, or complexes of ideal objects, which are called number-systems. The number-concept, which, as I before pointed out, is for the modern mathematician very prominently an ordinal concept, has become, in its various modern forms, something more general, as well as logically more fundamental, than the concept of quantity. Our exact knowledge of the laws of quantity thus tends, more and more, to appear as founded upon our knowledge of the laws of number, the latter being deeper and more universal. The result is the tendency towards what Klein has called the *Arithmetisirung* of mathematical methods. Now this *Arith-*

metisirung implies in part, making prominent, as I pointed out earlier in this paper, the ordinal concepts. But it also implies *giving a prominence to exactly defined classifications which I suppose has never before been known in the history of science.*

Our knowledge of number-system is, in very large measure, a knowledge that there are, in each system, these and these classes of numbers, and that of every number in one of these classes one can assert what one must deny of every other number in the system. Dedekind's famous and epoch-marking definition of the irrational numbers as corresponding to the totality of the classifications or *Schnitte* that one can make in the series of rational numbers, is one brilliant instance amongst many of the way in which classifications have become important in modern exact science. Another instance is Georg Cantor's definition of the grades, or dignities, the *Mächtigkeiten* of infinite assemblages of objects. The discovery of this new concept by Cantor seems to me one of the most brilliant feats of constructive imagination in recent times. It has enriched mathematics, and will enrich future philosophy, with wholly new views of the problem of the Infinite. Yet it turned upon a beautifully simple application of an exact principle of classification. Modern algebra, in the conception of what are called 'domains of rationality,' has again used an obvious and fundamental principle of classification, whose application to systems of numbers is very vast, and whose value in very various sorts of problems appears to be immeasurable. The most modern researches into the principles of geometry, and of the other exact sciences, in their efforts to find a sufficient and closed system of mutually independent first principles, have shown how much is gained by exactly classifying the ranges, or domains, to which various principles can be said to apply. Even the single principles, taken by themselves, appear, when thus examined, to be simply classifications of facts. Thus the principle that any two points in a space determine one straight line, while two straight lines can have but one point in common, is for certain purposes best stated as a classification of the points of space. These points namely are such that, if you choose at random any two of them, these two determine one class of

points such that every point in space either belongs or does not belong to that class, while no two classes so determined have more than one point in common. Thus stated, the principle regarding straight lines and points appears as it ought to appear; namely, it appears as no self-evident axiom, but as a surprising and even baffling property of the points in space, and so as an arbitrary fact of our spatial experience. It is as if you said: "There is a nation of men somewhere such that any two men in that nation belong to one exclusive club, to which every other man either does or does not belong, while no two such clubs have more than a single member in common." Such a nation would have a strange sort of club-life. But just such an assemblage are the points in space.

Classification from such a point of view reigns then everywhere on the highest level of exact science. Sharp classification is the goal as well as the beginning of the thought that gets embodied in the special sciences. To say 'yes' or 'no' to the question: "Does this object belong or does it not belong, for this purpose, to this collection of objects?" is the last as well as the first task of the human thinker in all his dealing with particular facts. Now the logical interest of this generalization about the nature of science lies in the consideration that, from this modern point of view, for which the special sciences, as you remember, are descriptions of phenomena, all our valid explanation of facts, just so far as they are valid, all our knowledge of the laws of nature, all our quantitative insight into things must be reduced merely to such classifications of facts, and to serially ordered systems of such classifications. Of such materials our conceptions of what I have called our world of description must consist. One modern writer has explicitly made this very generalization. I refer to Mr. A. B. Kempe, in his paper on the 'Theory of Mathematical Form.' Mathematics, according to Mr. Kempe, who illustrates his notion in a very varied way, is purely a science of exact classification, and is nothing else. It defines the relations of objects and systems of objects by classifying certain of these objects, or certain pairs, triads, or other groups of these objects, by placing certain of them together, and by distinguishing them

from other objects or assemblages of objects. Thus, according to Mr. Kempe, one studies geometry in a strict logical order by beginning with the conception that the points of space are, as mere points, undistinguished one from another. One then goes further and notes that not only all points, but all pairs of points in space, may be regarded as undistinguished from one another, so long as you ignore the notions of direction and distance. One next observes, however, that if one takes account of *triads* of points, one has forthwith a classification of such triads, because all collinear triads of points are distinguished from all non-collinear triads. Upon the basis of this primal classification, as Kempe holds, all the rest of geometrical knowledge can be built up by adding further classifications as new principles are introduced. Every new principle means merely a new classification. And this procedure, as Kempe holds, is typical of the processes of exact thought everywhere. Science, then, consists altogether of classifications.

Now what I want to point out is the enormous importance that such considerations give to the function which, in the life of our thinking, I have called the 'yes' and 'no' consciousness. This, I have said, is the consciousness wherein we are aware of accepting or inhibiting certain acts—acts through which we treat two or more objects as belonging to one class, or as belonging to classes that exclude each other. The contrast of X and not-X is always a product of the working of such a 'yes' and 'no' consciousness. Now I have said that psychologists have too much neglected the closer study of the 'yes' and 'no' aspect of consciousness. Psychologically speaking, it is that aspect of our mental life which accompanies our attitudes of readiness to perform certain deeds, and of attendant readiness to inhibit other deeds. Here then is a place where the modern logical inquiries counsel the psychologist to undertake a more careful study.

As a fact, classifications depend, for us, *upon inhibitions, and upon becoming conscious of our inhibitions*, and also upon bringing to notice the positive motor tendencies that are in us correlative to these inhibitions. Those who have studied abstract ideas as Ribot has done, or judgments as Marbe has done,

have therefore attacked the problems of the thinking process at the wrong end. They have tried to examine the corpse of a dead thinking process. They have found little left but a reflex act. Live thinking is the process of classifying our objects by suppressing, in their presence, certain of our possible motor acts, by welcoming, emphasizing, or letting go certain of our other acts, by becoming aware, somehow, *i. e.*, in some conscious terms, of these our positive tendencies and inhibitions, and by them regarding the objects in the light of the deeds that thus we welcome or suppress.

The most promising problem about the whole thinking process which is thus suggested to the psychologist may then be defined as this: "In what way, to what extent, and under what conditions, do we become conscious of our inhibitions?" Plainly the negative principle in consciousness, the *Geist der stets verneint*, is the constant accompaniment of all our higher, our organized, our thoughtful activities. It is the principle which makes exact classifications possible. And descriptive thought, in the light of these modern researches, means exact classification, and means nothing else so much. It is by contrast with our inhibitions that our positive motor processes get their precise conscious definition, as inhibitions of inhibitions, as tendencies to act by means of overcoming opposing considerations, and as assertions that are at once coördinate with, and opposed to, denials. Our abstract ideas are products of such an organized union of negative and positive tendencies. We can therefore understand the psychology of live thinking processes only in case we understand *when, how far, and under what conditions, inhibition becomes a conscious process.*

But now the psychology of the inhibitory processes—how vast a range of interesting phenomena, and how imperfectly explored a territory, does not this name suggest to us all? The world of the phenomena of primitive *tabu*, how fascinating it seems! Yet with *tabu* human thought about certain of the exact classifications, both of conduct and of truth, would seem to have begun. The pathology of our inhibitory consciousness, how interesting its complications—how important clinically—how significant from the humane point of view! Some years

since, in a paper on the case of John Bunyan, I tried to present to the members of this Association an instance of the descriptive psychology of an experience largely made up of pathological inhibitions, occurring in the early manhood of a great genius. You all know how rich is the clinical material for the study of such cases. But the experimental psychology of the consciousness of inhibition—here surely is another extensive, accessible, and comparatively much neglected, and at the same time perfectly definite and promising field of work. I have now tried to show you that modern logical inquiries, in emphasizing the central significance that the process of classification possesses in all grades of our thought, have made more evident than ever that upon an understanding of the psychology of inhibition must depend a great deal of our further advance in a knowledge of the psychology of the thinking process.

I conclude then by urging upon my fellow members (1) the problem of our inhibitory consciousness and (2) the before-mentioned problem of the psychology of our ordinal concepts, that is, of our consciousness of ordered series of objects, *as the two great tasks that are set before the students of the psychology of the thinking process by the results of modern logical inquiry.*

If anything that I have said shall tend to further the mutual understanding between workers in psychological and in logical research, I shall be amply repaid for my efforts in trying thus to state to you something of what I see in the present situation of logical inquiry; while you, I hope, may in that case be not wholly unrepaid for the tediously abstract and lengthy road over which, by your kindness, I have been privileged to lead you.

PROCEEDINGS OF THE TENTH ANNUAL MEETING OF THE AMERICAN PSYCHOLOGICAL ASSOCIATION, UNIVERSITY OF CHICAGO, CHICAGO, ILL., DECEMBER 31, 1901, JANUARY 1, 1902.

Report of the Secretary.

The tenth annual meeting of the American Psychological Association was held at the University of Chicago on Tuesday, December 31, 1901, and Wednesday, January 1, 1902, in affiliation with the American Society of Naturalists. As the same time and place had been set for the annual meeting of the Western Philosophical Association it was decided to make the meeting a joint one of the two associations and this plan was successfully carried out in the sessions of Tuesday morning and Wednesday.

President Royce of the Psychological Association presided at the joint meeting on Tuesday morning and President Thilly of the Philosophical Association occupied the chair at the meeting of the Experimental Section on Wednesday. Thirty members of the Psychological Association were in attendance at the sessions.

At the regular business meeting of the Association held on the 31st, the following was transacted. Election of officers for 1902: *President*, Professor E. C. Sanford, Clark University; *Secretary and Treasurer*, Professor Livingston Farrand, Columbia University; *Members of the Council to serve for three years*, Professor George S. Fullerton, University of Pennsylvania, and Professor G. T. W. Patrick, University of Iowa. The following new members were elected: Professor H. Heath Bawden, Vassar College; Professor George A. Coe, Northwestern University; Professor Edwin G. Dexter, University of Illinois; Professor J. J. McNulty, College of the City of New

York; Professor Walter B. Pillsbury, University of Michigan; Professor Walter D. Scott, Northwestern University; Professor Walter Smith, Lake Forest University.

Upon request of the President of the American Association for the Advancement of Physical Education that the American Psychological Association appoint a representative upon a Committee on Statistics and Measurements, Professor Cattell was appointed the representative of the Association.

Upon request that the association appoint a committee of one to coöperate with similar committees from other associations in collecting and preserving speech records of various languages, dialects and persons, Professor Sanford was appointed as such committee.

The report of the Committee on Bibliography was received and placed on file and as a substitute the council recommended the following which was adopted: That a committee of five be appointed by the President, and that this committee be instructed to report at the next annual meeting of the association upon the subject of a psychological bibliography, including contents, plan of arrangement and of publication, and in their report to take into consideration all available existing material. The President appointed the following to serve as such committee: Professor Warren, Chairman, and Professors Sanford, Creighton, Sneath and MacDougall.

The Council also recommended that members of the American Psychological Association living in any center may, with the authorization of the Council, organize themselves into a local section for the holding of meetings. This recommendation was adopted and the establishment of branches in New York, Cambridge and Chicago was authorized.

REPORT OF THE TREASURER FOR 1901.

Dr.

To balance at last meeting	$1,222 53
To dues of members	363 00
To sales of Proceedings	25
	$1,585 78

Cr.

By expenditures for

Printing	$25 69	
Postage	18 25	
Stationery	2 00	
Clerical assistance	12 50	
Expenses of Baltimore Meeting	2 88	$61 32
Balance		$1,524 46
Interest on deposits (approximate)		120 00
Total		$1,644 46

Audited by the Council.

LIVINGSTON FARRAND,
Secretary and Treasurer.

ABSTRACTS OF PAPERS.

Address of the President: *Recent Logical Inquiries and their Psychological Bearings.* By JOSIAH ROYCE.

(The address appears in full in this number (March) of the PSYCHOLOGICAL REVIEW.)

The Interpretation of Savage Mind. By JOHN DEWEY.

(The paper appears in full in this number of the PSYCHOLOGICAL REVIEW.)

The Theory of Induction. By FRANK THILLY.

Some writers distinguish between scientific induction and unscientific induction, but regard both as forms of induction (Bacon, Mill, Veitch, Lotze, Wundt). Others reject the unscientific form or simple enumeration, and accept only that phase of induction which derives from particular facts the law of their necessary connection (Sigwart, Ueberweg, Bosanquet, Shute, Hamelin, Hibben, Creighton). Of these some identify induction with scientific method in general, including the forming of hypotheses, deducing their consequences, and verifying them (Sigwart, Jevons in 'Principles of Science,' Hamelin). According to some thinkers, only so-called perfect induction is certain; imperfect induction is only probable. Nearly all agree, however, that induction is grounded on the principle of the uniformity of nature. This principle is interpreted differently by

different thinkers, sometimes merely called by another name. Some speak of it as the principle of identity: what is once true will always be true; whatever is will remain so; the world is identical with itself (Lotze, Kromann, Bosanquet). Some express the same idea by saying the particular is the expression of the universal (Aristotle, Hegel). Some call the principle the principle of necessary connection; the given is necessary (Sigwart, Ueberweg, Hibben, Welton, Creighton). Some identify it with the law of causation (Mill, Jevons, Veitch, Benno Erdmann). Moreover, the principle of the uniformity of nature is conceived by some as a postulate of our thinking (Sigwart, Lotze, Kromann, Bosanquet, Hibben, Welton, Creighton), by others as the product of experience (Mill, Jevons, B. Erdmann).

The author's conclusions are: (1) Hasty and imperfect induction is just as truly induction as scientific induction. (2) Induction is not limited to the discovery of causal relations. (3) Induction does not discover only the inner, necessary relations of things. (4) Induction must not be identified with scientific method in general, for this includes both induction and deduction. The logical thing to do is to restrict the term induction to the process of inferring a general truth from particular instances, and to use another name for the combination of this process with deduction. (5) It is not true that we base ourselves on the principle of the uniformity of nature in induction, that is, that inductive inference is really deduction. Induction consists in making the so-called inductive leap, which must be regarded as a natural function of the mind. The principle of the uniformity of nature is a late product of experience, the result of induction, and not its ground.

On the Relative Frequency of Ideas. By CHARLES S. MINOT. (No abstract received.)

The Psychology of Causality. By W. B. PILLSBURY.

For the psychologist causality is a mark which attaches to two successive events and sets them off as related in a different and more intimate way than if they are merely successive. There are two questions which must be raised to define and

explain this relation. The first is what is the nature of the characteristic mark of causality, the second to determine the mental conditions under which it attaches to the successive processes.

The mark of the causal relation is shown by introspection to be largely made up of a feeling of strain or effort which is attached to object thought as cause. This anthropomorphic tendency to ascribe activity like our own to objects which are regarded as effective seems to pervade all thinking. Words, such as exert, pull, force, which are found on every page of works on physics all bear testimony to the widespread influence of personification, in our idea of cause.

The second aspect of the problem is still psychological, and we need to consider, not the question of the absolute truth of the causal connection, but merely the truth relative to the individual consciousness in which the two events occur. For an answer to this question as to the conditions under which the mark of causality appears we must look not to the frequency or intensity of the connection as did Hume and his successors, but to the influence of the related ideas. If we translate Bosanquet's treatment of the system of knowledge from absolute to concrete terms, and make it apply to the individual experience instead of the abstract universe, we should obtain a statement of the nature of the relation. And it must be possible to make the translation, for the elements of knowledge which he unites into a system are in the last analysis but mental processes of some kind, in spite of his violent protestations to the contrary. When the process is conscious the causal relation is confirmed by reference to analogous relations already regarded as causal. But much of the process is unconscious, and then the process owes its origin to the reinforcement of related experiences themselves unconscious at the time. The solution suggested would amount to a translation of Bosanquet's system of knowledge into Stout's apperceptive systems.

A Method of Measuring Mental Work. By C. E. SEASHORE.

In what sense can we speak of measuring mental work? What significance and value do such measurements have?

These two questions were discussed in the introduction. Then followed a description of an instrument and methods of measuring with it. The instrument has been given the descriptive name 'psychergograph' because it is used in making graphic records of mental work.

The aim in the designing of the psychergograph was to devise means by which one can (1) call forth a relatively simple and definite complex of mental activity, (2) repeat the same for any length of time, without interruption, and (3) measure (*a*) the amount of work done, (*b*) the time taken, (*c*) the quality of the work, and (*d*) fluctuations in speed and quality. As an example of the kind of work that may be measured, we may mention a case of simple discrimination denoted by the following setting: Given one of four known signals, to recognize it and make the corresponding one of four simple responses.

The psychergograph consists of two distinct parts, the stimulator and the recorder. The stimulator makes a series of quick exposures, the order of the signals being determined by chance. It has four reaction keys, each bearing a signal for selective reaction. Each reaction brings out a new signal and the process may be repeated without interruption as long as may be desired. The recorder furnishes a continuous tracing of the action of each of the keys in the stimulator; also a time-line. The record is made on common telegraph tape by electro-magnetic pencils. It shows, along a time-line, the duration of each act as well as the time of the whole series. It also shows the number of errors and the nature of each error. From the experimenter's point of view the operation is completely automatic. He has only to press a button to start the recorder and give the signal to begin. The personal equation of the experimenter is therefore eliminated. The records are permanent and may be read at leisure.

The record thus obtained is supplemented by description of the condition of the observer, by an introspective account rendered by the observer after each test, and by observations made by the experimenter during the test. The psychergograph gives an accurate, unbiased record, but the significance of this depends upon our ability to account for the conditions which are elements in the process measured.

A great variety of measurements may be made. In fact, all the complications of the usual reaction experiment may be introduced with the additional possibility of uninterrupted repetition of the same process for any length of time.

(A full account will appear in Vol. III. of the University of Iowa Studies in Psychology.)

Class Tests in Psychology. By JOSEPH JASTROW.

Many of the tests devised to determine the functional efficiency of a given process or group of processes would acquire greater applicability if they could be so arranged as to be applicable to a class of individuals instead of to each individual separately. This situation appears as a practical problem more frequently in collecting data in the schoolroom than under any other special set of circumstances. If one can collect data upon forty individuals in the same time as upon one the gain is sufficiently obvious. Hence it seems worth while to inquire what are the general requirements that will transform the one type of test into the other. The first is that the test shall be self-recording. In a great majority of individual tests the function of the experimenter is largely that of a recorder. If the record itself yields the material needed for the calculation of the results, then the test becomes a class test. Frequently a slight modification is needed to bring about this desideratum. In questions involving time tests, it can be accomplished by measuring not the time needed for a single performance of a simple or complex reaction but the quantity of such reactions that can be consecutively done for a given period. A second principle is that a simplification of apparatus is required; this not only because it must be duplicated as many times as there are individuals in the class, but equally because a device that shall be handled by the uninitiated must be simple. A satisfactory test of the judgment of lengths of lines can be obtained by printing triangles all of which have two sides alike in length but a third side differing more or less from the other two, and then requiring that the unequal side shall be indicated. The same test can be arranged for the sense of touch by having triangles cut out of cardboard, which are to be placed in a box with the short side upwards.

With simplification of apparatus and an automatic record of the result by tabulation of the amount of work done, by indication of errors, by marking off certain selected traits of a sense-impression, by recording by position or arrangement, etc., a large range of tests can be reduced to the class type. Even where an apparatus can be handled only by one person at a time, results may be quickly accumulated if it be arranged that the directions are explicit and the machine is self-recording. In some cases, too, it is possible to make the test in couples, allowing one person of each couple to act in turn as subject and as experimenter or recorder. The psychologist, like all other workers, must be saving of men and of time; there is no more pressing demand upon him in this respect than economy in the arrangement of tests so that the accumulation of results shall not involve too extravagant an outlay in material, time, or assistance. The class test is a step in this practical endeavor.

The Effects of Practice on Illusions. By CHARLES H. JUDD.

This paper reported experiments in which two subjects made a large number of quantitative determinations of the strength of the Müller-Lyer illusion, for the purpose of discovering the effects of practice on the perception of this illusion. For both subjects the illusion disappeared in about 1,000 determinations, although one subject expected the change and the other was wholly ignorant of the purpose and results of the experiment.

In the case of both subjects, the effects of practice with one figure were transferred to a variety of different figures. One special case of such transfer is of interest. The subject who did not know that the illusion had disappeared showed a very pronounced increase in the illusion when the standard line was transposed from the right to the left side. This negative transfer of practice was more significant because it showed no tendency in a long series to weaken or disappear.

(This paper has appeared in full in THE PSYCHOLOGICAL REVIEW for January, 1902.)

Mental Imagery of Students. By F. C. FRENCH.

(This paper has appeared in full in THE PSYCHOLOGICAL REVIEW for January, 1902.)

The Duration of the Auditory After-Sensation. By MAX MEYER.

The first attempt of measuring the auditory after-sensation was made by Alfred M. Mayer in 1874 with seemingly full success. The result was that the duration of the after-sensation was inversely proportional to the vibration frequency. In 1898 another method was employed by Abraham. His result was that the duration of the after-sensation was constant, *i. e.*, entirely independent of the vibration frequency. One of the two methods, therefore, must be fundamentally wrong.

Mayer's method was this (neglecting various smaller modifications): The tone was produced by a tuning fork and conducted through a tube to the ear of the observer. The tube was intersected by a disk with a row of openings, so that at rotation of the disk the tone was heard intermittently, but when the velocity of rotation was sufficiently increased the tone appeared smooth, because—as Mayer assumed—the after-sensation was as long as the time interval between two of the beats.

Abraham used an entirely different method. The tone was produced by a siren of which alternately a number of holes in a row were open and closed. At a first glance it seems possible to increase also in this case the velocity of rotation until the interval between two beats is as short as the after-sensation, when the tone should be smooth. However, the tone will never be smooth in this case, as the intensity of each beat does not abruptly begin and end, but rises and falls in a certain manner. A series of such rising and falling tones cannot appear as a smooth tone unless the fall of each preceding and the rise of each succeeding tone be perfectly symmetrical, which is quite improbable. Under these circumstances we cannot make use of smoothness in order to determine the duration of the after-sensation. Abraham, therefore, used a siren with two rows of holes, producing two different tones, and being so arranged that one tone is sounded while the other pauses, and *vice versa*. At rapid rotation of the siren a trill is heard, but when the pauses are filled up by the after-sensation, no trill is heard, but two *simultaneous* tones. The unavoidable roughness is then

without any consequence, and the result is that the after-sensation is constant, *i. e.*, independent of the pitch.

The wrong method is the one used by Mayer. That the tone becomes smooth when the rotation is rapid, is not caused by the pauses being filled up by the after-sensation, as Mayer assumed, but by an entirely different condition. When the rotation of Mayer's disk is rapid enough, one vibration of the tone of the fork will pass through unobstructed, the next one will be weakened by the disk, and so on alternately. Then, of course, the tone sensation cannot be alternately strong and weak. Two impulses at least (in a higher region a few more) are physiologically necessary for the production of a tone sensation. If one of these two is great and the other small, not a succession of a strong and a weak tone is heard, but a single tone of invariable intensity. There is no cause at all for a fluctuation of intensity, and the tone is smooth; but no duration of an after-sensation is measured by this observation.

We now also comprehend Mayer's—unfounded—assertion that the duration of the after-sensation was inversely proportional to the vibration frequency. If we take a tuning fork an octave higher and wish to let one vibration pass through the tube unchanged, the next one weakened, etc., we have of course to increase the velocity of rotation twice; but this does not permit any conclusion as to the duration of the after-sensation in the case of this higher tone. *I. e.*, the seemingly beautiful method of Mayer turns out to be no method at all.

The Theory of Rhythm. By ROBERT MACDOUGALL.

(This paper will appear in full in an early number of THE PSYCHOLOGICAL REVIEW.)

A Biological View of Perception. By THADDEUS L. BOLTON.

The inapplicability of the old categories of psychology to modern experimental and comparative methods is generally recognized. The purpose of this paper is to revise one of them in the light of some ideas borrowed from biological study. Much that enters into the perception of an object has been overlooked. The older psychologists proceeding by the method of

analysis have penetrated only so far as to discover the elements contributed by the direct afferent currents initiated by the object presented. These are the most obvious as well as superficial elements. The active part of perception needs to be emphasized, the part which arises through the reaction of the organism upon the object. When we trace perception down in the animal scale to its earliest beginnings, we find it generally fading into automatic and instinctive performances. Perception reduced to its lowest terms is an act and as such it is synonymous with instinct and emotion. Objects that arouse no instinctive or emotional response are perforce unperceived by animals. Even in man this acting in view of objects still enters largely into the perception of them. Perception is, therefore, an attitude towards objects. Beginning as perception does in instinctive performance, percepts must be regarded as more or less refined emotions.

Mental and Moral Heredity in the Royal Families of Europe. By FREDERICK ADAMS WOODS.

The following is an abstract from an extended investigation to disclose the strength or weakness of heredity as a factor in the formation of character in a large number of interrelated individuals where known pedigrees and characteristics are obtainable and open to the verification of anyone who should question their exactitude.

By taking all persons present in complete family records there is no selecting of instances to support any theory. Also the opportunity to obtain sufficiently complete records of all on the maternal side is an advantage over any previous work. The inclusion of mediocrities and those of low intellectual standing as well as geniuses is of interest, since it shows the heredity effects of all the different classes of minds on each other. The study of various moral qualities in their relation to heredity has never been carried on to any extent in a systematic way with complete pedigrees, and considerable light has been thrown on this question through a study of Royalty. The houses included are the following: Hanover in England, Saxe-Coburg, Saxe-Saalfeld, Brunswick, Hohenzollern, Denmark, Russia, Sweden,

Savoy, Saxony, Spain, Portugal, France, Netherlands, and the Hapsburgs in Austria and Italy. The years covered extend in general back to the sixteenth century, through four centuries further in the countries Spain and Portugal. 564 persons have been correlated for intellectual grades and 487 for moral. Since, in general, the same person occurs first as a remote descendant, then as a near one and subsequently as an ancestor, and since there is considerable intermarriage, these figures have much more value for scientific purposes than their size would at first indicate. Besides this, as a separate investigation, the relation of genius to mediocrity has been studied among 3,312 non-selected interrelated royal and noble persons whose scientific value, owing to repetitions, is estimated at 32,768.

The persons were ranked in ten grades in relation to each other, one number being given for intellect and one for morality, virtues of any sort being included under this head. A number of authorities were used in estimating each individual, all adjectives were taken down and it was found contrary to the expected that the authorities only rarely disagreed with each other and usually on the less essential aspects. Attention was paid to the law of deviation from an average. According to figures given only a few are in the very high or low grades.

The results show in every separate country an incontrovertible argument that heredity is the main if not nearly the entire cause of the intellectual eminence which these men and women have achieved in relation to their fellows.

The remarkable curve of distribution supporting Galton's law, and falling off from the geniuses where the qualities have not been kept up through fortunate unions, cannot be due to environment as some may contend where different social grades are compared as in the studies of Galton (Hereditary Genius) and Ellis (British Genius). Everywhere is noticeable the following principle which, as far as I know, is new and should be coupled with Galton's law when studying mental or moral heredity—that the hereditary intellect of a person is not likely to represent a blending of the various ancestral traits, but is likely to take largely, though not entirely, from some one of his various ancestors, usually a near one, less and less occasionally a remote.

The results on the moral side are compatible with the theory of heredity, but it is more difficult to separate the influence of environment. However, the applications of the above principle to the countries separately and comparatively have led to the conclusion that here also heredity is probably the main cause. Free will has apparently been of little or no consequence as a cause for their achievements.

The World and the Individual. By W. CALDWELL.

This paper was a critical study of Professor Royce's two volumes of Gifford Lectures. What R. gives us is a basis for the philosophy of religion in a theory of being founded upon an interpretation of human experience. His volumes constitute the most extended and the most closely reasoned expression of the philosophy of dynamic idealism that we possess in English. He begins with the motor aspects of mental processes and (in metaphysic) with the accredited results of Kant's critical method, being obviously anxious to overcome the well-recognized defects of 'Neo-Hegelianism.' R.'s theory of being (that it is invariably 'meaning') is at the same time also an epistemology and a teleology. There is thus for him no *hiatus* between being (*i. e.*, 'meaning') and knowledge (*i. e.*, consciousness of 'meaning') and end (*i. e.*, fulfilled 'meaning'). R. gives a better account than does Münsterberg of the 'World of Appreciation' in relation to the 'World of Description.' He also derives important epistemological consequences from the influence of the 'social factor' in knowledge. R. may be criticised for his manner of treating his assumptions, and in not always being explicit enough regarding them. In particular we need to make his thought consistent, a philosophy that shall logically unify his 'World of Appreciation' and his 'World of Description.' R. in admitting that 'the individuality of all things' is still a 'postulate,' 'something that our ethical consciousness demands,' seems to imply that philosophy (in its 'World of Appreciation') is using the hypothetical method just as science does (in its 'World of Description'). What therefore is the relation of metaphysic to experience—determinative of it or determined by it?

Æsthetic Categories from the Standpoint of Social Psychology. By JAMES H. TUFTS.

Among the more generally accepted categories of the æsthetic are: (1) Universality or objectivity or shareableness; (2) disinterestedness, detachment, semblance or make-believe; (3) human or typical significance. We may be made to explain these from the standpoint of individual psychology, *e. g.*, the objectivity of æsthetic value may be attributed to the fact that the eye and ear, which are preëminently the æsthetic sense organs, tend to objectify all qualities apprehended through them. Admitting the partial adequacy of this method, I maintain that although the two judgments, 'I like it (or, it pleases me),' and 'It is beautiful,' may both express æsthetic feeling, the second or objective judgment implies a social reference; universality and objectivity are thus different aspects of the same attitude. The explanation of this social reference may be approached from the standpoint of social psychology.

I. The æsthetic consciousness in its beginnings is connected with art rather than with nature.

II. The relation of the æsthetic (appreciative) consciousness to art is not that of cause, but that of effect. Art has not arisen chiefly to gratify an already existing love of beauty either in the artist or in the public. It has arisen from various other springs—economic, protective, sexual, military, magical, ceremonial, and religious. In some of its forms it is closely connected with plays, but plays usually represent an experimental or tentative expression of some instinct which finds its roots in the serious activities of adult life (Groos). Hence the early products of the related arts are presumably due to instinctive initiative. Art in all its forms may thus be said to have created the taste by which it is now appreciated.

III. The origins of art as enumerated above are almost without exception social. Art has been produced for social needs, and fostered by social occasions. It has served social ends in the struggle for existence. It has been enjoyed by social groups. The pleasurable stimulus of color and sound has been heightened and reënforced by social sympathy. Rhythm is at least powerfully reënforced by common activity even if not

wholly due to it. Æsthetic feeling may then be expected to show its origin in its characteristics.

The considerations presented apply especially to the categories of groups (1) and (3) above. The explanation of disinterestedness or detachment from reality may be sought in the social psychology of the process by which certain serious pursuits have become sports or games. The explanation, so far as it applies to the social reference implied in objective æsthetic judgments, is analogous to Royce's deduction of the categories as necessities of description. Bergson has given an account of the comic from the standpoint of social psychology.

The Epistemological Limitations of Ethical Inquiry. By NORMAN WILDE.

The study of ethics rests upon certain logical principles, common to it with the other sciences, which determine the nature of its problems and its method of explanation.

1. No science has to prove the existence of its own subject matter, but assumes it as part of the common experience of the race, its problem being the understanding of a given material which it neither constructs nor deduces, but finds. So ethics has not the task of deducing or constructing morality, but of analyzing and interpreting an actually given moral experience whose reality is a matter of fact, not of theory.

2. No special science has to prove the possibility of a knowledge about its material, but approaches it directly under the supposition that such knowledge is possible. The scientific problem is as to what is the actual law of any given set of phenomena, not as to whether there *is* such an intelligible law. Similarly ethics has no need to raise the problem of the intelligibility of moral experience, but may proceed at once to the investigation of the assumed moral cosmos. It is no greater an assumption and no less necessary that we live in a common world of ends than that we live in a common world of objects. As rational beings we act upon the supposition that there is an ideal order of experience discoverable by thought both in the spheres of fact and of value, a supposition without which there would be neither knowledge nor conduct.

3. All explanation consists in the exhibition of the common principle involved in any set of particular instances, by means of which these instances are shown as members of a systematic whole. Scientific concepts are the symbols in which the unity in experience is expressed and must bear definite relation to the experience to be explained, varying with each change in the subject matter studied. Distinct kinds of experience demand distinct sets of explanatory symbols. Moral experience consists of judgments of a better and worse in conduct and its unit is not the scientific judgment that A is the cause of B, but the moral judgment that A is better than B. Ethical explanation must consist in the exhibition of the system of such judgments and its symbols must be those of value and not of fact. The concept of causality has no more significance for ethics than has that of obligation for physics. Scientific method in ethics, therefore, though it consists of observation and analysis of actual moral experience, involves the use of explanatory concepts other than those of causality.

Epistemology as an Independent Discipline. By GEORGE MARTIN DUNCAN.

Some Characteristics of the Genetic Method. By EDWARD FRANKLIN BUCHNER. (Read by title.)

The discussion of method in psychology must be regarded as meaning something more than the mere adaptation of instruments to data. The intimate nature of mental science is often disclosed in an examination of the postulates and implications of method, rather than by the declared conceptions elaborated within the science itself. Method thus becomes interpretation; and scientists cannot too frequently ask themselves: How are we going to think in psychology? With all their variations, methods must not be regarded as external to the body of doctrine which they support. The genetic method has been pressing itself forward as the only true procedure in preparing and attacking the problems of psychology. Its usage has proceeded without careful determination of its *differential;* yet it has had a long history. The adoption of the concept 'function' has heralded the application of the method.

Five groups of characteristics of the method are considered. The genetic method is distinguished for its attitude towards psychological material, its differentiation from brain, stimulus and experiment psychology, its disposition of the acute problem of psychological causation, its claim of being the highest and final method of the science, and the variety of assumptions upon an acceptance of which rests the validity of its adoption. The method is extremely selective in accepting its working material from the lower, instinctive, automatic processes, and rejects all but the modified process theory of the soul. It fosters a brain psychology thinly disguised, does not readily unite with the standpoint of psycho-physics, and must, in fine, not lay broad claims to the sole advantages of experiment. The problems of 'causation' are to be removed from psychology absolutely, in order to make room for the substitute conception of genetically related stages; and, true psychological knowledge is to tell us about soul ages, rather than about soul processes. Analysis and the quest for causal explanations are thus eliminated from the science. The method also uniquely regards itself as disposing of the antithesis between a science and its method by uniting them into one final production. All of the foregoing alleged achievements for the science are really dependent upon a number of assumptions. The genetic method is also to be distinguished for its conspicuous relation to error-liability. While it seems that the method is thus subject to very severe criticism, it possesses, on the other hand, many excellent features, which render it valuable for increasing true knowledge.

Shakespeare and Schiller: A Study in Apperception. By BROTHER CHRYSOSTOM. (Read by title.)

Some Aspects of the Religious Motive and the Salvation Philosophy of Schopenhauer and of Von Hartmann. By J. H. LEUBA. (Read by title.)

Abstracts of papers read by members of the Western Philosophical Association will appear in the Proceedings of that association.

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THE INSUFFICIENCY OF MATERIALISM.

BY PROFESSOR G. S. FULLERTON,

University of Pennsylvania.

It must ever remain a matter of regret to those who are imbued with the scientific spirit, and who love clear thinking, that the works of Democritus were allowed to perish. When one has wearied one's wings by soaring in the empyrean with Plato; when one aches in every joint after an agonizing struggle with the Aristotelian conceptions of matter, form, moving cause, and final cause; one turns with a sigh of relief to the simpler and clearer teachings of the ancient materialism. The system is easy to understand; its outlines are distinct and may readily be followed by the eye. It reveals itself to one frankly and openly, standing naked in the light of day, stripped of that veil of ambiguous words and unintelligible expressions with which philosophic systems are wont to drape themselves. It informs us that nothing exists save atoms and void space. These atoms differ from one another only in size, shape, and position. They have always been in motion. Their mutual collisions result in mechanical combinations from which are born world-systems, with their varied phenomena. Nothing comes from nothing; nothing becomes non-existent. The cosmic changes are but translocations of material particles, and this truth may be grasped by the reason, though the senses are too dull to furnish direct verification of it. The universe is a universe of matter in motion, a gigantic mechanism, the successive steps in whose development form a limitless chain of causes and effects in no ambiguous sense of those words. The whole of science is summed up in the comprehension of this order of causes.

That Democritus was an unblushing dogmatist, and cheerfully described in detail all sorts of things of which he could have no possible knowledge, seems sufficiently evident. There is a striking difference between the easy birth of the atomistic

doctrine in ancient times, and the protracted labor which resulted in the atomic theory as we have it now. The old world was uncritical, and cheerfully optimistic as to what could be accomplished by speculative thought. The modern world is more cautious, and has a somewhat better realization of the magnitude of its task. Hence the ancient atomism can easily be criticised in detail; and yet its bitterest assailant cannot fail to see that it has grasped with marvellous clearness an idea in which men of science are more and more coming to rest, the idea of the world as a mechanism, the life-history of which is summed up in an unbroken chain of mechanical causes and effects. The teachings of Democritus, modernized in form and rendered a trifle less dogmatic, would not be found to be much out of harmony with what has been said in the preceding paper [1] touching the occurrences which take place in the material world.

I say expressly, touching the occurrences which take place in the material world, for that paper has concerned itself only with matter and the motions of matter, ignoring the existence of anything beyond. The ancient materialism lays down for itself, it is true, the same limitations; but it undertakes, nevertheless, to say something about minds and their knowledge of things, a field of investigation which it can call its own, as we shall see, only as the result of an act of violence which rebaptizes the minds and ignores the existence of their knowledge altogether. Mind is composed of fine, round atoms, and is disseminated through the body. Atoms are discharged from external objects, pass through space to the organs of sense, and mechanically affect the mind; thus arises the knowledge of external things. This doctrine, as it was later developed in detail by the Epicureans,[2] is highly ingenious and, to men at a certain stage of their reflective development, can scarcely fail to be attractive. It differs only in unessentials from the type of doctrine with which we frequently meet to-day, in men of science who have paid little attention to philosophical disciplines and are unacquainted with the history of speculative thought. They do not speak of mind-atoms, but there is much talk of the external

[1] THE PSYCHOLOGICAL REVIEW, Jan., 1902.

[2] See Lucretius, De Rerum Natura, III.

stimulus, of the organ of sense, of the sensory tracts, of the central nervous system, of the motor reaction. There is also a tacit assumption that with an exhaustive investigation of all these, the whole field is covered. Yet it is clear that both the ancient and the modern materialism simplify their task by dropping out of sight what is most obscure and elusive, and fixing their attention exclusively upon what is comparatively easy to grasp. If mind-atoms differ only in size or shape or mobility from other atoms, if they have their location in space, it is easy to conceive how they may be jarred into new motions by the impact of atoms cast off by surrounding objects. There is nothing hopelessly mysterious in the clash of material particles; we see something of the kind going on about us on a larger scale all the time. But if we are to be content with this view of the process of knowing, we must pass lightly over the very significant statement that "thus arises the knowledge of external things." Nothing exists save atoms and void space; under which of these heads shall we subsume this 'knowledge'? or shall we, perhaps, make it identical with the motions of the atoms through the space? And if we drop the notion of mind-atoms, and confine ourselves to the study of nervous processes and those physical events in which they have their inception and in which they terminate, the case is the same. What becomes of those phenomena with which the psychologist supposes himself to be dealing? What becomes of sensations, memories, thought-processes? A whole world of things seems to be left wholly out of account, ignored as though it were non-existent. Shall we outrage common sense by insisting that these are but another name for the nervous processes themselves, and hence do not require independent investigation?

The absurdity of such a position can best be made clear by the use of an illustration. Let us suppose the boy, whose motions have been discussed in the preceding paper, to be about to begin his attack upon the dog. As we have seen, boy and dog are certain collocations of material particles in certain space relations to each other and to the rest of the material world. They are part of the mechanical system of things. Every motion of every particle is foreordained by the law

of the whole, and could be foretold by one sufficiently well informed and sufficiently wise. To us, the spectators of the drama, the actors do not seem to be such swarms of minute elements, but Democritus could inform us that this is because our senses are too weak to see them as they are. Suppose that by some miracle this hindrance were removed, and that boy and dog stood revealed to us in their atomistic nudity—infinitely complex, discontinuous, each a universe in which system could be traced within system, all developing their countless series of changes in harmony with mechanical laws. Could we see all this as it would be open to the eye of omniscience, the task of science, in so far as it is merely physical science, would be satisfactorily completed. Every change in every particle of matter, and, hence in every collocation of particles, would be accounted for. We should know perfectly why the boy hits the dog, and why the dog runs though his series of twistings and turnings. Puffed up with such knowledge we might feel inclined to despise the blind antipathy to Dr. Fell that remains incapable of justifying its existence by a reference to mechanical causes.

But while we are thus gazing upon the intimate structure of the boy and the dog, we become conscious of the fact that the closest acquaintance with the machine does not bring within our view certain things that we might have expected to find there. The boy sees the dog, and sees him to be yellow. He hears him bark. What are these sensations of color and sound? What have they to do with the mechanism? They are certainly not a part of it in any intelligible sense of the word. The machine and all its workings can be perfectly well understood without referring to them at all. To our discriminating eye the vibrations in the luminiferous ether and the vibrations in that grosser medium, the air, lie open and are numbered. The mechanical changes, the translocations of atoms, which take place in the organ of sense—changes which an observer endowed with a vision less acute could only subsume under such concepts as chemical or 'vital'—stand forth stripped of their mystery. The subsequent changes in the sensory nerves, the rearrangement of atoms and molecules in the central nervous system, the changes

in the motor nerves and in the muscles, all these we follow step by step. The chain of mechanical causation is unbroken, and it is nowhere necessary to turn aside from the straight path upon which we are journeying. Nowhere do we find color or sound, or anything resembling color or sound. The more clearly one realizes just what is meant by the world as mechanism, the more clearly does one see that it is a world which has in it no room for a vast number of things which are plainly to be found in our experience, and the existence of which can only be overlooked by one blinded by prepossession in favor of some philosophical theory.

Upon the crudely unreflective materialism which rather startled the world with the emphasis of its unmeaning utterances half a century ago it is scarcely necessary to comment to-day. The much discussed statement that the brain secretes thought as the liver secretes bile needs no labored refutation. To such vision as we are supposing ourselves to possess, the mechanical structure and functioning of each organ would be plainly evident. The secreting organ and the secretion would in each case be perceived to be such and such collocations of matter, having an unequivocal existence in the material world of things, and no single atom or molecule in either would lack its definite place in the mechanism of the universe. The globule of saliva is as much a part of the material world as is the salivary gland. The atoms which compose it have an existence as independent as the atoms which compose any other group, and they are equally indestructible. Their relations to the atoms in every other group are spatial, and all changes in these relations may be described as motions in space. The gland and the secretion may be separated and set at a distance from each other; this does not affect the existence of the secretion. The gland may be destroyed, that is, the collocation of material particles which passes by that name may be made to undergo great change; nevertheless the secretion may remain unaffected. The relative independence of gland and secretion, and the unmistakably material nature of the latter are thrust unpleasantly upon our attention by the numberless threats and admonitions which the constituted authorities in civilized countries have found

it necessary to affix to the walls of waiting-rooms in railway stations, to hang up in trains, and to bring to our notice in divers other places. The ill-bred fellow who has been lounging in the corner of the railway-carriage, takes his salivary glands with him when he steps out of it; but he leaves behind an unwelcome reminder of his former presence, which persists in its independent being and asserts its right to a place in the world of matter. Can any thoughtful man seriously maintain that the color seen and the sound heard are related to the brain of the boy, who sees the dog, in any way analogous to this? The man who sat in the corner might have occupied himself during his whole journey with thoughts of wholesale massacre; he might have called before his imagination the most hideous combinations of colors; he might have hummed over in his mind the most unmelodious of tunes; yet, on his exit, the place might have been taken contentedly by a timid man with artistic tastes. Of such things as these no trace remains, and no one expects to find a trace. Sounds, colors, and a whole world of other things that we may classify with these, are not collocations of matter which exist in space side by side with certain other collocations of matter which we call bodily organs. It is only mental confusion that can identify them with such.

Perhaps some one will be tempted to point out once more that the functioning of the brain does result in certain material products which can be traced by the physiologist. There is a destruction of tissue which must be made good by reconstruction. This is, of course, true. When the brain functions, there are waste products which pass into the blood and are ultimately eliminated from the body by other organs. But it should be noted that such products, when they are discovered, are not found to be in the least like those things which we have been discussing. They are not colors, they are not sounds, they are not memories of such. They are not to be identified with any of those things of which the man was conscious while his brain was functioning. The elements which compose them formed part of the man's body; they were jostled out of the combinations in which they stood; they were finally excreted. Of their existence during the whole process he has not had the faintest

suspicion. For identifying them with the things of which he was conscious at the time there seems to be no excuse.

Thus this vain talk of 'secretions' may be unhesitatingly set aside when we are considering such things as the color of the dog as seen by the boy, or the sound of his bark as heard. Even the Democritean slurring over of the existence of sensations and that of reason which can alone discern the truth about the atoms and their motions seems preferable to such gross misconception. Democritus recognized the existence of these things, but failed to find for them a place in his scheme of existence. The secretionist gives them a place in the system of things, but they cannot take that place without ceasing to be what they are. He denies them their own proper nature and confounds them with something else.

It may be thought that it is an excess of zeal to spend even so much time as I have done in the criticism of this form of the materialistic doctrine. Why sally out in chase of the dodo, when that bird has disappeared from the face of the earth? To this one may answer that *this* bird has not wholly disappeared, but that specimens may still occasionally be met with in out-of-the-way corners. My own experience has been that they are more apt to be found in the medical profession than elsewhere, perhaps because that profession embraces a vast number of men who have some acquaintance with physiology and psychology, but only a limited number of whom can be legitimately expected to be possessed of philosophical acumen and to be thoroughly equipped with accurate information upon matters physiological and psychological. And one may answer, in the second place, that the secretionist's misconception is but one of a type, and it may serve to throw light upon a whole group of errors to analyze the most striking instance to be found in the group. A more insidious form of the misconception is often made to lurk in the statement that what is somewhat loosely called thought is a 'function' or 'activity' of the brain, a statement which may seem not unsatisfactory to one who is ready to turn a deaf ear to all mention of secretions. One is reminded here of the old Greek notion of the soul as a harmony of the body, which notion, as readers of Plato will remember, was some-

times taken with serious literalness and supposed to be fraught with grave significance.

But it is never wise to use a phrase without at least an attempt to determine with some accuracy what it really means. What are 'functions' or 'activities' of the brain? To such vision as we are supposing ourselves to possess, it is quite clear what the brain is. The dullness of our sense has been done away, and we see, as with the Democritean Reason, an army of atoms going through its evolutions with mechanical precision. It is not a mob, a mere rabble. We can trace in its infinite complexity relatively permanent groupings in the midst of incessant changes. Formation succeeds formation; the individual units group themselves, divide, scatter, and re-form into new groups. A patient observation of what takes place, and a comprehension of the mechanical laws which govern the actions of each, enable us to predict what groupings will appear upon the scene when the present arrangement has filled its moment and dropped into the nothingness of things past. These motions in matter, these groupings and regroupings of atoms, these are the functions or activities of the brain, in an unequivocal sense of the words. They are the only ones that display themselves before our eyes, and, as we have seen in the preceding paper, they are the only ones needed by science to explain the whole series of positions taken, in the material world, by the body with which this brain is connected — in the instance above mentioned, the wild chase of the dog, the shouts of laughter, the wavings of the stick. Shall we say that the color seen and the sound heard are also functions of the brain? And in this case shall we regard them as distinct and separate functions of a quite different kind, or shall we assume that they are identical with some of the motions which we see before us? Shall we say that this particular clash of atoms is the color yellow, and that one is a sound? If we assert that such as these are functions of a quite different kind from motions, we seem to be stretching a familiar word to the point of breaking. We ought to recognize that, when we call things quite different by the same name, we are not justified in putting them into the same class, and in assuming that the one has been assigned its

place in nature when the other has. On the other hand, if we maintain that colors and sounds are identical with certain atomic motions, we seem to be talking nonsense. The atomic motions we can see plainly before us. As well call a triangle an emotion of grief as call this particular clash of atoms yellow. The atoms are not yellow and their motions certainly are not. If it is an error to confound a color or a sound with a material secretion, it is surely no less of an error to confound them with motions in matter.

As a matter of fact even those who elect to speak of thought as a function of the brain, do not exactly identify colors and sounds as seen and heard with motions in the constituents of the brain. They do not conceive those motions to be colored or resonant. They accept their own phrase loosely, and when cross-questioned usually have something to say about double-faced entities, the outside and the inside of things, etc. With these modifications of their doctrine we are not here concerned; what concerns us is the fact that any doctrine which maintains that science has to do only with matter in motion removes from the province of science many things which common sense and common experience insist upon as really existing. If science is to be thus circumscribed, then scientific knowledge carried to its extremest limit must wholly ignore much that we find in our experience, so much, indeed, that, were it dropped out altogether, we should not recognize our experience as our experience at all.

The more clearly one recognizes, therefore, just what is meant by the mechanism of nature, the more clearly one sees that there is no room in it for such things as color and sound as seen and heard. This world of mechanism is, indeed, the world of the primary qualities of matter dwelt upon by John Locke in his 'Essay.' From it all those elements of our experience which are sometimes loosely called the secondary qualities of matter are to be carefully excluded. Colors, sounds, odors, etc., are not, as Locke expressly states, qualities of matter at all, and he insists that they do not resemble them. That something in matter must correspond to them, he regards as self-evident, and this something he calls the secondary qualities of

matter. But he defines these secondary qualities as powers which objects possess of arousing sensations in us by means of their primary qualities. Thus, in the world of matter, there is no real distinction between primary qualities and secondary. The secondary are seen to be nothing other than the primary—they are configurations of, or motions in, matter; these particular motions which we connect with, and too often confound with, the hearing of sounds or the seeing of colors. That such configurations and motions should not be confused with the sounds heard or the colors seen Locke saw clearly. He made the latter effects of the former, but he had better sense than to suppose the two classes of things to be identical.[1]

The modern man, who has had the advantage of reading what men have written since touching the nature of our conception of matter, ought to be in still less danger of falling into such confusions. The world of matter and motion is a world given in terms of touch and movement sensations. It is a vast system built up out of elements which have been selected from our experience as a whole, but which by no means exhaust its rich diversity. It is a mere skeleton, a framework and nothing more. When it is recognized what the material world is in its ultimate constituents—I speak psychologically and not physically—it is impossible to think that nothing exists save matter and motion. This is seen to be tantamount to the assertion that color sensations are identical with sensations of quite another class, which is palpably absurd.[2] To regard as identical classes of experiences which are evidently dissimilar is inexcusable, and to dismiss as non-existent all classes of sensations except those which fit into a particular series, arbitrarily narrows the meaning of the word existence to a special use. Both in science and in common life we constantly speak of colors, sounds, and odors. We mean something when we do so. To declare such things to be non-existent is palpably contrary to common sense and to the accepted usages of speech.

[1] Book II., Chapter VIII.

[2] If anyone chooses to distinguish between the material world 'as given in terms of touch and movement sensation' and the real material world as it is, distinct from all sensation, it does not affect the question. It only emphasizes the absurdity of overlooking the existence of the 'subjective.'

Thus we see that it is impossible for reflection to rest content with the Democritean world of atoms and void space, and ask no questions touching those other things which Democritus recognizes but to which he explicitly denies a place in the system of things. It is impossible to be satisfied with a mechanical theory of the universe, however carefully elaborated by modern science, which simply ignores a large part of our experience, and regards its task as completed when it has reduced to order the remainder. One is constantly reminded that something remains to be explained. In common life we hear little of the atomic structure of things, and much of the color, the odor, the taste, of the apple or the peach. We speak of our wine as white or red, as sweet or sour. A bruised finger aches, and all notion of mechanism is driven from our thought by its maddening pulsation. These things stand in the foreground of our experience; to overlook them seems absurd. To think of the world as composed exclusively of atoms in motion, one must banish the world, sit quietly in the dim light of one's study, glue one's eyes to the paper, and write oneself gradually into a frame of mind in which the abstractions of mechanics seem the only realities. The first tap at the door, the first note of the finch in the tree outside, may easily remind one that the world is really painted in colors, and is not a monotony of black and white.

It is the same when one talks with men of science, or reads an account of their experiments. We watch the chemist pour one colorless liquid into another. He has told us that the 'resulting color' will be this or that, and his prediction seems to have been justified. The physiologist gives us a brief sketch of the anatomy of the eye and of the ear. He traces as well as he can their connections with the various parts of the brain. He then launches out into a far more extended discussion of sensations of color and sound—not brain-changes, but sensations of color and sound—as though such things really existed, were worthy of being discussed at prodigious length, and were not so cut off from molecular changes in the substance of the brain as to make it impossible to pass from the one to the other. As for the psychologist, whatever may be his enthusiasm for mechan-

ism, and however closely he may ally himself to the student of physical science, he simply cannot speak at all without reminding us that there are other things in heaven and earth than motions in matter, than the clash of the Democritean atoms. If we expunge from his pages all reference to what does not form part of the mechanism we have been discussing, we leave most of them as white as when they went into the hands of the printer. Even the headings of the chapters are gone, and the title of the volume has become an empty sound. There remain some descriptions of apparatus, and an outline of the anatomy and physiology of the nervous system, the latter a mere shadow of its usual self as we find it set forth in the works of the physiologists.

Very likely it will be objected that this devastation which is wrought in the sciences by insisting that they shall omit all reference to what cannot take its place in the world of matter and motion, has its origin in the fact that the sciences are as yet so imperfect. A science which does not know the actual changes which are taking place in the mechanism of the universe, must, if it is to talk at all, be allowed to talk about something else. Yet he who thus speaks may be conscious of the fact that, did he know more, he might speak in quite another way. The pouring of one liquid into another is a mechanical change. The chemical combinations which result may also be regarded as mechanical changes. Such changes, which, of course, do not lie open to direct inspection, may be assigned their place in the cosmic series of causes and effects. One may speak of the 'resulting color' without seriously intending to maintain that the color seen has its place in the series. It may be taken as merely representative of what has such a place, as a convenient handle by which to take up an occurrence which cannot readily be laid hold of in some better way. It is permissible to refer to 'Monsieur Chose' when we do not know the man's real name. Similarly, our desolating ignorance of the intimate structure of the brain and of the changes which take place in it, may force the physiologist and the psychologist to talk of colors, sounds, odors, tastes, pleasures, pains, memory-images, concepts, and what not; but if they knew more of the

mechanism of the human body, could they not describe all its activities without any reference to such things as these at all? Were science more advanced, could there not be a physiology, and even a psychology, that made no reference to such? Could not these sciences study man as a mechanism, and content themselves with the knowledge of all that this mechanism could possibly do? Certainly, if the mechanical view of the material universe is a true one, it is not permissible to follow the chain of mechanical causes a little way, abandon it at a certain point, and then return to it again, except as a last resort and a temporary expedient. One may deplore this expedient even while availing oneself of it.

To the objection that the chain of mechanical causes and effects could, at a more advanced stage of science, be rendered more evidently complete, one need not care to bring an answer. I have merely wished to point out the fact that, in the present state of the sciences, it is inexcusable to overlook the existence of all save the Democritean atoms and their motions, since that existence is forced upon one's attention at every turn. Nor is it without significance that it is possible, when we find the series of mechanical causes broken by our ignorance, to piece out its deficiencies by turning to something else. Certain things cannot be made to stand as representatives of certain others unless there be some true relation between the two classes.

The importance of this relation is sufficiently evident, for it is possible for the plain man to interpolate into his series of mechanical causes such things as sensations, and yet to infer with a good deal of accuracy what occurrences will or will not find a place in the world of his experiences. It seems to him madness to deny that sensations and volitions can be the results and the causes of changes in the material world. The puncture caused by the mosquito gives rise to the sensation of itching, and this sensation leads to his scratching the spot attacked. The fall of the apple from the tree causes in him certain visual sensations, and these visual sensations are the cause of his desiring to possess the apple, which desire sets his body in motion and leads to the appropriation of the fruit. The descent of the

hammer wounds his finger; this causes pain; the pain causes facial contortion and the insertion of the wounded member into his mouth. The fact that such chains of antecedents and consequents do present themselves within his experience, no man can with justice deny. He assumes them to be a series of causes and effects, and he regards it as unnecessary to isolate and set apart the merely material, even if the thought of doing so ever crosses his mind.

The man of science is apt to speak with rather more hesitation, even when he makes no deliberate attempt to view things with the eye of the philosopher. The chemist may talk of a 'resultant color,' and may even admit frankly that he thinks of color as an effect of physical causes, but we do not find him ready to admit that color can in any true sense be a cause of physical changes. The physiologist tells us that a common effect of the arrival at the central nervous system of impulses passing along afferent nerves is a change in consciousness, or a sensation.[1] He also tells us that choice may be determined in some cases by intelligence,[2] and that in an ordinary voluntary movement an intelligent consciousness is an essential element.[3] He assures us, on the other hand, that, looking at the matter from a purely physiological point of view, "the real difference between an automatic act and a voluntary act is that the chain of physiological events between the act and its physiological cause is in the one case short and simple, in the other long and complex."[4] Psychologists divide themselves into classes, the one class falling in with the opinion of the plain man and the other regarding the series of mechanical causes as unbroken. One cannot claim the authority of psychologists as a class for either doctrine. Finally, the logician tells us that it is the great aim of science to trace the relations of cause and effect which obtain in nature, but we remark the fact that he does not hesitate to illustrate the inductive methods of scientific research by a description of investigations into the 'causes' of the iridescent colors on mother-of-pearl, or on thin plates and films.[5] We

[1] Foster, 'Physiology,' 6th ed., III., pp. 850, 851.
[2] *Ibid.*, p. 909.
[3] *Ibid.*, p. 1068.
[4] *Ibid.*, p. 1004.
[5] Jevons, 'The Principles of Science,' Chapter XIX., § 2.

ask at once, does the logician mean to maintain that colors have their place in the natural order of causes and effects? Can they be the result of mechanical causes? Logicians speak as though they could, and they treat them accordingly.

Of course, the adherent of the doctrine that the material world is a perfect mechanism will regard those whom I have above cited as in need of enlightenment. He will maintain that the opinions of the plain man must not be uncritically accepted as true; and will point out that one may be a pretty good chemist, physiologist, psychologist or logician, without on that account being much of a philosopher. He will, moreover, call attention to the fact that, in special investigations of all sorts, it is permissible to use language in a way which is not strictly correct, provided that such a use of words serves our convenience and does not give rise to unavoidable misconception; and he will remind us that one may reason well without being fully conscious of the true significance of the terms employed in one's reasonings. Those who enjoy the clearest vision, he will insist, and who best understand the course of the development which science is undergoing, will be in the least danger of falling into the error of supposing that the cosmic mechanism really needs to be patched with such unsubstantial stuff as colors or odors, pleasures, pains or memory-images.

But when he has said all this, he ought to frankly admit the significance of the fact, that such wide-spread error may exist without either in common life or in science revealing itself to be error by undeniably undisastrous consequences. This can only mean that those things which he has set aside as finding no place in the cosmic mechanism are, after all, intimately related to that mechanism. Where our knowledge of the mechanism is defective, it may be more or less satisfactorily pieced out by their aid, as we have seen. And it is quite clear that were our knowledge of the world of matter and motion so complete as to make it quite unnecessary to borrow such patches, this would not in the least imply that the world of sounds, colors, tastes, odors, and all the rest, would cease to exist and to be related to the world of matter and motion. In certain special investigations it would, it is true, be unnecessary to refer to such things,

whereas this reference is at present unavoidable. But to limit the sphere of science to such investigations seems absurd. It is surely not the whole duty of man to fix his attention upon the ordering of sensations of touch and movement into a satisfactory mechanical system, to the complete neglect of experiences of every other sort. That these other experiences do not defy all attempts at arrangement is sufficiently clear from what has been said above. It seems, then, as though it ought to be the task of science, in the broad sense of that word, to reduce the whole of our experience, and not merely a part of it, to some sort of system. Anything less results in the mutilation, not the explanation, of the world in which we live.

But how attain to such a view of the whole of our experiences as an interrelated system? Surely one may sympathize with the Democritean, and admit that he is driven to his position by encountering what seems a very real difficulty. Once admit that the material world is a perfect mechanism, and there appears to be no bridge by which one can pass from it to another world and back again. To the plain man the difficulty does not exist, for his real world is a composite thing in which material and non-material elements are patched together to form what cannot exactly be called a mechanism, and yet resembles one in spots. To the nature of the connections between its different and discrepant elements he has given little thought; that they are somehow connected is enough for him. But he who desires to think clearly can scarcely rest content with a conception which seems to remain satisfactory only so long as it remains vague and obscure. He asks how he is to conceive this connection of the material and the non-material, and what is meant by their interaction. The more he thinks about the thing, the more it seems to him impossible that motions in matter should have as their causes anything save motions in matter. And yet, if this be so, what shall one do with colors, sounds, odors, and the rest? What shall one do with the subjective, with *mind?* Has it a place in the system of things, or has it not? As the 'system of things' is pretty sure to mean, to one who has busied one's self chiefly with physical science, the cosmic mechanism, an exclusion from the latter may seem almost tantamount to a denial of existence.

Such a denial is manifestly mystifiable, and can scarcely be made by a man with open eyes; but one may glide over the subject lightly, as the atomists appear to have done, and discourse chiefly of the material. Or one may half face the question, and justify one's exclusive occupation with the material by the assertion that thought is a bodily secretion, an assertion which we have seen to be a foolish one, and one which testifies rather to a man's respect for the mechanical order of things than to his powers of reflection. Finally, one may regard mental phenomena as 'inside' of molecular change, or call matter a 'double-faced' entity, thus seeming to connect things of divers kinds which do not seem capable of being built strictly speaking into the one system. Just how much one may mean to say, when one uses such expressions, must depend upon one's clearness of vision. They may only indicate a vague recognition of the existence of the world ignored by the Democritean, coupled with the desire to incorporate it somewhat equivocally in the world of matter in motion. They may, on the other hand, mean more, and they deserve careful analysis. But the mere fact that one is tempted to use them is a sufficient indication of a recognition of the futility of attempting to limit the sphere of science to a description of the changes which take place in the material universe. It is an admission that something exists save matter and motion, and a doctrine that makes this admission has advanced beyond the standpoint of pure materialism. It may, it is true, remain materialistic in feeling, and the amount of attention it bestows upon the subjective elements of experience may be quite inadequate. Still, it should be given credit for a truth which it sees but dimly. If it sees it at all, it cannot conscientiously object to the most strenuous efforts to throw light upon this dark corner in human knowledge. It cannot, in other words, frown upon the labors of the metaphysician, unless this worthy makes it quite plain that he assumes his premises without proper precautions, uses words and phrases without having carefully looked into their significance, draws conclusions without clearly recognizing what constitutes proof, or does any of those things that have so frequently made the word metaphysician stink in the nostrils of the prudent and the practical man.

His task is not an imaginary one. It is set for him by the nature of our experience. Even Democritus unconsciously incites him to set about its accomplishment, in that he delivers into his hands certain things which unquestionably exist, in some sense of that word, and yet for which no place is provided in the world of existing things.

DISCUSSION AND REPORTS.

PROFESSOR FULLERTON ON 'THE DOCTRINE OF SPACE AND TIME.'

In a series of articles published last year in the *Philosophical Review* Professor Fullerton has discussed in an interesting way the doctrine of space and time. His articles bear the following titles: I. The Kantian Doctrine of Space; II. Difficulties Connected with the Kantian Doctrine of Space; III. The Berkeleian Doctrine of Space; IV. Of Time; V. The Real World in Space and Time; and with regard to the terms Kantian and Berkeleian we are promptly told that these are used 'in rather a broad sense to indicate types of doctrines,' there being no intention 'to make either Kant or Berkeley responsible for later additions to or alterations in the structure which he reared upon the foundations that he himself laid down.' As might be inferred from the titles, Professor Fullerton's object is to adapt the two opposed views to each other, but as it turns out with a strong preference for the Berkeleian. Thus (p. 385):

> "Surely the Berkeleian doctrine is preferable to the Kantian and should replace it. But it is desirable not to overlook the fact that the latter doctrine emphasizes a very important truth—it insists strenuously upon the validity of the application of mathematical reasoning to phenomena. In this it is wholly in the right, for here it is recognizing the system of relations which obtains within our experience as a whole. Its only error—that is, its only fundamental error—lies in supposing that in dealing with any single intuition it is dealing with 'real' space and 'real' things. If the Berkeleian will admit that 'real' space is infinitely divisible (as it may be), and if the Kantian will admit that 'real' space is not given in any intuition (as it certainly is not), there need be no quarrel between them."

And what is said of space *mutatis mutandis* is said in substance also of time, so that in the foregoing quotation, which is at once a summary of Professor Fullerton's criticism and the foundation of his subsequent construction, we have—if the metaphor is not too sanguinary—an excellent base for the operations of the present review. But before mobilizing our forces it will clear the ground a little to say that there seems to have been no real need of the *finesse* about those terms used 'in rather a broad sense.' The Kantianism and the Berkeleianism that are discussed seem peculiarly free from anything like neo-ism.

Now, for our first move, Professor Fullerton's study, in so far as a comparative study, is open to the same objections that would be urged properly enough against partial or garbled quotations or against the abstract comparative study of the corresponding members of any two creatures in the animal world. Animals are living wholes and to abstract certain parts for comparison may afford a useful exercise, say in anatomical investigation, but it can have only the value of exercise or at least it cannot have the value of fundamental comparison. Moreover the objections involved here are, if there is any difference, more pertinent in the case of Kant's and Berkeley's philosophies than in that of anatomical study, for the criticism of these men is certainly very far from the exercise stage. Am I reminded that there are nevertheless real defects especially in Kant's doctrine of space and time? Then I must insist (1) that Kant's philosophy is a living whole both in the matter of the relation of its many parts and in the matter of its own development and (2) that even in the event of conspicuous error in any part a good deal is to be said for the notion of any thoughtful philosophy—such as Kant's!—being a system and if a system also a system which in some way direct or indirect itself compensates for its own errors. On both of these counts, then, the vital wholeness and the inner compensations, I find reason for qualifying the value of Professor Fullerton's study. Can his criticism afford even to seem unconscious of either of them?

But, secondly, if so far I may be accused of being hypercritical, making such labors as Professor Fullerton's too complicated and too arduous even for the present day, with the following suggestion no one can find fault. Any up-to-date study of space and time, even of Kant's and Berkeley's space and time, or of the 'real' world in space and time, can not afford to be made in any light but the best which the current information of the sciences, especially of mathematics, is capable of supplying. In the articles before us, however, one finds little if any sense of the message that the newer mathematics, to say nothing of some other sources of information, brings to the question at issue. Of course it is always easier to detect than to supply a defect, and I would make no pretenses myself to being up-to-date mathematically, but for present purposes it is enough that in a doctrine of space and time one should at least try to be. Questions of infinity, of infinite divisibility and the like which Professor Fullerton properly enough and perhaps conventionally enough treats as of paramount importance in his undertaking are if not directly at least indirectly questions of mathematics, and the implied if not the open solution of them in

mathematics can not but affect—in a way that I, having caught some of the rumors of the day, shall try to indicate—those other questions, also considered by Professor Fullerton, of 'real' space and time being intuitions or not or of the really intuited space and time being only symbolic.

Thus Professor Fullerton very early in his discussions runs against the old-time paradoxes of divisibility and infinity and he certainly says some excellent things about them. In italics, for example, that are his own he declares (p. 237) that '*the whole nonsensical edifice*' which the paradoxes and the traditional uses of them, including the Kantian, have made—'*rests upon the one nonsensical assumption that an endless series can be completed by a progress which results in the attainment of a final term.*' This is certainly true or an important part of what is true, but within only a page or two Professor Fullerton betrays what is at least one of its logical consequences in that he misses the real import of infinity by recognizing the possibility of infinite divisibility and accusing of mere quibbling those who would distinguish between being *infinitely divisible* and *infinitely divided.* What can the absence of that final term, supposed to end the endless series really mean if not that infinity is not a quantity at all, that the infinitely small part is a contradiction of terms and that accordingly infinite divisibility must refer, if there be any meaning however hidden or indirect in it, to something besides physical or quantitative composition. Being *infinitely divided* is a conception of status and is obviously impossible, while *infinite divisibility* is neither static nor quantitative. Infinity is a door by which both quantity and division or composition enter into a new world and become transfigured.

Parallel lines on a plane surface meet at infinity just because they are parallel, the meeting at infinity being really significant of their quality or relation—the parallelism, not of their length. Two such lines of only an inch 'meet at infinity' even within the inch in having a *common* straightness, the infinity being only an indirection for the relation. Circles in the same plane have parallel circumferences and they all meet at infinity in the same way.

The series constructed from the results of a continued bisection, 1, ½, ¼, ⅛ . . . etc., is completed only at infinity and has a determinable sum, not because it has a final term, but on the contrary because, as Professor Fullerton himself insists, there is no final term. Yet this is very far from meaning infinite division as a status or possible status; it means only that infinity is an indirection for the constant relation of bisection, that even with the first division and the consequent derivation

of the second term all that the infinity itself stands for has been expressed. The infinitely small or great taken quantitatively is only symbolic of a uniform process, of an activity under a fixed law, of a principle immanent in every term of the series and giving to the series as a whole a unity that quite transcends the limitations of quantitative division.

Professor Fullerton's disc (p. 238) affords still another case. It completes one revolution in half a second, the next in a quarter of a second, etc., etc., and at last develops an infinite speed, which means or seems to mean no time at all for a revolution and is accordingly equivalent to rest. But the infinity means only the uniform acceleration, there being in it, not the rest of a final term, but the rest of the uniformity which dwells within the process itself and so belongs to all the terms. In short, in this case as in the other cases, infinity stands as an indirect but not less effective way of asserting the constructive principle of a series and so also of symbolically presenting in a quasi-static form the dynamic character that the series contains or even more generally the quality of any number-group. The infinity is immanent in every one of the numbers or every one of the terms.

In the history of mathematics did not application of the idea of infinity eventually turn quantity from mass to ratio? But in the ratio quantity somehow becomes independent of the differences of mere magnitude. The ratio is a law, not a mere sum. With the ratio, however, in course of time there came and there had to come into mathematics the equation—really another gift of infinity. The equation is only the full expression of the law. At infinity, once more, that is, in an infinitely divided space, the flying arrow rests, Achilles runs hopelessly, and that disc stops, but in each case emphatically the rest is no mere negative of extensive, distance-covering motion; it is instead the constancy of some relationship, as if the intension of motion. In a world of quantity as mass motion is motion, but it is rest in a world of quantity as ratio and being rest there it has been open to mathematical treatment through the equation and the calculus that has the equation and infinity for its corner stone.

So those old-time paradoxes and the nonsensical edifice for which they are responsible rest upon something more than merely the nonsensical assumption of a final term. They are equally and perhaps more significantly—although I should not draw invidious distinctions—due to blindness to the import of infinity. The real last term at infinity is not indeed one more among all the other terms and coordinate with them; rather it is the abstracted principle, say the sheer

parallelism or the bare fact of bisection—without anything to be bisected, or the simple constancy of acceleration or the mere persistent ratio of Achilles' to the tortoise's speed, that sets or establishes the series, and in the light of the history of mathematics this is but to say that the paradoxes and their edifice are consequent upon nothing more or less than the two meanings of quantity which infinity bridges—quantity as mass and quantity as ratio, the one static and the other dynamic. Duplicity always makes paradoxes and if blind it makes nonsensical paradoxes.

But now what of Professor Fullerton's distinction between 'real' space and time, which are infinitely divisible and are not intuitions but constructs or conceptions, being 'the plan or system of [the real world's] actual and theoretically possible relations and changes' (p. 598), and intuited space and time, which are not infinitely divisible and are only symbolic? "The world as it lies before me," says Professor Fullerton, "is * * * not a thing directly given in intuition, even if I stop at the world of common knowledge, and refuse to follow the scientist into the unseen region in which atoms and molecules disport themselves in a space infinitely divisible. What is intuitively present in consciousness is not enough to constitute such a world. It can only represent it. It is, indeed, the *symbol*, and the world is the thing symbolized. * * * We *think* [the world], that is to say, there is intuitively present in conciousness that which represents it, but that is all we can say" (p. 587). To say to Professor Fullerton that his distinction breaks down would not be quite fair or true, for if we grant the general standpoint of Kant and grant accordingly that the only pertinent questions are 'questions of fact' as to what is intuited and what is not intuited but conceived in our space and time experience, then among the answers of Kant and Berkeley and Fullerton, who are all on the whole under the same spell, the answer of the last is decidedly the most satisfactory and is well supported by the author himself. But the form of thought, the general standpoint, not the pigeon-holing of the material, is what needs important if not radical revision and the material meanwhile may be left to take care of itself. To touch upon nothing but that of which Professor Fullerton himself makes so much in his argument, the question of infinity and divisibility is solved by him in a way that in the first place is *formally* Kantian and that in the second place, as has been shown here, at least seems to be wholly blind to the double meaning of quantity. Accordingly, his eyes being thus darkened he is able to dream as he does and along with his other contentions to say what he certainly does say so well (p.

589 sq.) of possible void spaces and times, but the duplicity, when recognized, changes the whole view as if by projecting it upon an entirely different plane. Massiveness—mere finiteness—is not the only truth of quantity; instead, if taken alone it is really an untruth; but relationship or ratio—infinity—is another and a fulfilling truth of quantity. All quantity is *both* mass and ratio and is one of these in being the other, not one *and* the other. The world, then, of intuition can hardly be a world intuited only in a finite space and time and serving as a *symbol* of reality in an infinite and infinitely divisible space and time. The intuited symbol must itself be in and of the reality, however infinite and impossible to consciousness—of course a formal, static consciousness—this may seem to be; or, in other words, intuition, like its quantitative forms, must have a dynamic character, and in this character alone, not in any other basis of generalization, can its so-called symbolism lie, while through this it includes, not is merely accompanied or followed by the activity, the immanent activity of thought. Are the *given* forms only symbols of life to the morphologist? Or of force to the physicist? Or even of religion to the real worshipper?

In recent mathematics we have a development that is not without its special significance here. Mathematics has been making wonderful excursions apparently away from this world of space and time altogether into other worlds, strange and unrecognizable, the worlds in general of other dimensions than those with which in our positive intuitive consciousness we have any familiarity. And what can such excursions mean? Hardly a reality of which this world is symbolic, for those other worlds are no mere extension of certain fixed, statically intuited forms of this. They are worlds, not of mere distances and magnitudes, however great or small, but of altogether non-spatial and even non-temporal rational orders; they are worlds of specific arrangements; they are systems, their dimensions being only so many functionally related variables. And so, unless—as is not impossible—my ears have heard the rumors falsely, they are after all actually in and of this world of our positive consciousness. I would even be so bold as to say that infinity by involving if not clearly revealing the principle of ratio or relationship in the world of quantity had been at least the outer door through which mathematics had entered these new worlds. Yet is this new mathematics 'applicable to phenomena'? It has done more or bids fair to do more for the cause of application than whole cycles of tridimensional formulæ, and its applicability must be taken as conclusive evidence of the relation between intuition and thought, symbol and symbolized, being far more intimate than even Professor Fullerton seems ready to admit.

But, in conclusion, for some of the things that I have said I may be classed with those whom Professor Fullerton accuses of 'bringing metaphysics into disrepute with men of scientific mind' (p. 598). I can only say that the rather long-standing fear, so mutual and so cordial, between science and metaphysics seems to me humorous 'to a degree'—forgive the lapse. There are times when boys stand up before each other and talk abusively and boastingly but safely keep their distance, and science and metaphysics have been very like them. But for my part, whatever the consequences, I believe that more courage from both might accomplish some good things. There is only one reality. Why two independent studies of it? The scientific mind is, indeed, a noble thing, but truly mind is a nobler.

Alfred H. Lloyd.

University of Michigan.

ON THE 'FATIGUE' OF NERVE CENTERS.

In a brief but interesting article in *The New York University Bulletin of the Medical Sciences* of July, 1901 (Vol. I., No. 3), Dr. R. S. Woodworth maintains that, contrary to the rather common opinion, the nerve-centers controlling voluntary muscular movements are not more easily 'fatigued' than are the muscles connected with them—and this seems to be very slowly indeed. From both the physiologic and terminologic points of view this contention seems open to some criticism of a doubting sort.

The author bases his opinion in this matter on four portions of evidence which may be summarized as follows: (1) "The fatigue of voluntary muscular contraction is very much slower in developing than Mosso's [well-known] curve would indicate. * * * I have myself made a series of over 1,300 maximal contractions lasting in all for three-quarters of an hour, with a loss of but 10 per cent. of the original force." If the whole motor apparatus concerned in these movements shows this great endurance, surely, thinks the author, the nerve centers involved cannot be as easily fatigued as Mosso and many followers suppose. (2) Experiments were made intended to compare the fatigue curves produced by the muscles alone and those made in cases where the motor nerve centers also were employed. Simultaneous records from a frog's two gastrocnemii muscles were made, one being electrically stimulated directly and the other electrically stimulated through the medulla or else through a sensory nerve. "I was surprised to find no perceptible difference in the rate of fatigue" by these two opposed methods. Similarly, experiments made on the human subject (by

comparing curves made by stimulating the muscles directly with those made by muscles voluntarily innervated), 'gave sensibly the same rate of fatigue.' From these two sets of experiments the conclusion was suggested that 'the fatigue of the nerve centers was practically *nil*,' within the limits of the experiment. (3) In those experiments which 'call for the closest attention and most exact coördination, but not for great muscular effort,' there is opportunity for the studying of fatigue of nerve centers free from the complication of muscular fatigue. For example, when a person hits a dot target on a table before him with a lead pencil the movement can be repeated 12,000 times with a loss of only 13 per cent. of the original accuracy. In correcting examination papers even six hours at a time, the degree of fatigue was small compared with what one would expect. "In this sort of experiment, as well as in voluntary muscular work, there is an abundance of *feeling* of fatigue; but the feeling apparently does not indicate actual weakness and loss of efficiency on the part of the nerve centers; the test for true fatigue is the quantity and quality of the work done. (4) The work recently done by Joteyko (in *Comptes Rendus Soc. de Biol.*, 1899, 384) bears out Dr. Woodworth's contention apparently. This researcher separated central from peripheral muscular fatigue by interposing a galvanic or ether 'block' in the course of the motor nerve and then subjected the center to prolonged stimulation. The block being then removed, the muscle was immediately innervated, showing that the center had not been exhausted by its exertion.

"The conclusion from all these tests is that the nerve centers—both those of the spinal cord and those of the brain hitherto examined—so far from being quickly burned out or paralyzed by their own activity, are exceedingly resistant to fatigue. They are, in normal conditions, capable of a large amount of work without suffering loss of power."

Now it seems obvious that there is in this brief consideration of the endurance-capacity of nerve cells, in the first place, a confusion of terms, and secondly a disregard of certain psychophysical considerations which change the aspect of the apparent results of the experiments, and so forth, cited.

It is highly probable that what is commonly known as fatigue Dr. Woodworth discriminates as 'the feelings of fatigue,' and that what he calls 'fatigue' (meaning thereby organic, cytologic 'fatigue') would be technically and accurately known as exhaustion. This confusion of common usage with precise scientific meaning has happened frequently—as indeed Professor Lee pointed out in a recent

meeting of the American Psychological Association. This seems an instance where harmful confusion might arise from this misusage. The nerve-cells have strictly of course no fatigue in themselves, for they have no feelings of any sort, but they do become exhausted progressively, however slowly, in cases where their blood supply is ample and their vigor normal. Still, it seems good physiology to suppose that when any particular functional group of motor cells has performed a certain muscular innervation the habitual number of times, there should develop in them a distinct strain, and that this strain of acute falling energy should be, perhaps by cortical radiation, transmitted to consciousness as a distinct and acute unpleasantness, merging often when the work done has been excessive into a paralyzing pain. Such indeed seems to have been the case in fatigue studies like those of Mosso.

It is, then, the individual who experiences the fatigue, and as good examples one may cite the subjects in Mosso's famous experiments and in those of all others who have made these fatigue curves since he made them first. These felt fatigue proper, and their organisms reacted to it in the normal, psychophysiological way. Dr. Woodworth's experiments, on the other hand, are purely physiological, for he expressly disregards the psychic side of the experience, as if it were unconcerned with the question.

It is common knowledge that nerve-cells are in exceedingly close and quick sympathy with their supply of energy, the blood and lymph, and they, therefore, only after long periods of exercise get behindhand in their balance of energy and so become exhausted, in the normally vigorous individual. In the anæmic or the neurasthenic person all recognize that the case is far otherwise.

In the long series of endurance movements described by Dr. Woodworth the psychophysical conditions are probably more complex than apparently is presumed in the present contention of the author. It is the common experience of the subjects in these experiments, *e. g.*, of the present writer, that true fatigue of the neuro-muscular mechanism involved comes on comparatively soon (in fact, just as Mosso's curves indicate that it comes on), and one is impelled spontaneously then, as Dr. Woodworth says, almost 'reflexly,' to rest the weary limb. This feeling, this fatigue, is a protective concomitant of the customary limit of this particular activity. The author describes it thus: "It is, in fact, a very complex affair. It is compounded largely of fatigue sensations from the eyes, neck and other parts of the body that are under strain, of feelings of ennui, of impulses to do something else,

and of the habit of stopping work after a certain time." Now this feeling comes on early and shows itself regularly, if one is not mistaken, in the curves as a more or less distinct period of inaccuracy and more or less incoördination. *Can it be doubted that this arises chiefly in the motor cells involved in the act*, the disturbance there (a temporary acute minor exhaustion) being parallel to pain in the parts concerned? Soon the nerve cells get, so to say, their second wind, and the fatigue passes off or only after a long time becomes altered into the far different, much vaguer but deeper and more persuasive feeling which accompanies exhaustion of the cells concerned, muscular then as well as neural. The feeling of fatigue seems far too acute to be accounted for otherwise than as of central stimulation referred, as usual, to the peripheral part concerned.

Thus the discussion turns, but in an important way, on a matter of terminology, for the present writer is by no means aware that it is held as a common opinion by physiologists, etc., that *exhaustion* of the nerve centers is ever a result of ordinary exercise of any sort. Exhaustion is recognized very generally as a condition of serious import, not easily brought about (save by distinct disease) and only with considerable difficulty overcome. Thus it is not to be doubted that should a voluntary movement for example be repeated for twenty-four hours instead of for one hour or two, the neuro-muscular apparatus concerned would be paralyzed in earnest. This would be true neuro-muscular cell exhaustion, having absolutely nothing to do with fatigue as such. That which is regularly called fatigue, on the other hand, is probably a psychic dissonance from the over-pushed 'voluntary' or the motor cells, coming soon and easily dispelled.

George V. N. Dearborn.
Tufts College Medical and Dental Schools.

RAPID MEMORIZING, 'WINGING A PART,' AS A LOST FACULTY.

Many years ago I was interested in collecting data on memory and had some correspondence with actors in regard to the former practice of rapidly acquiring or memorizing a part. Among many letters I received one from Mr. Harry Edwards, then connected with Wallack's Theatre, which seems of so much interest that it should be preserved in print. It relates to a faculty which is now no longer cultivated in this period of long theatrical runs. The letter has an additional interest because Mr. Edwards, as an avocation, collected moths and was a man of considerable scientific reputation. The su-

perb collection which he brought together during his tour in different parts of the world is now preserved in the American Museum of Natural History.

HENRY F. OSBORN.

WALLACK'S, NEW YORK, February 23d, '84.

My Dear Sir:

I have been so much occupied with the business of the theatre since I received your note, that I really have not found a moment to answer it until now.

The faculty of "cramming" a part, or, as we call it, 'winging' it, *i. e.*, learning it at the wing, is undoubtedly only to be acquired by practice and long experience, and is in these days of long runs unknown amongst our younger actors. When the bill of the night was changed much more often than is now the case it was a matter of necessity that the words of a part should be quickly acquired, and it was then no uncommon occurrence for a man to take a part in the morning and play it at night, reading it as he came off the stage at every scene and fixing mechanically upon his memory the shape of the written part, the very hand-writing, the position of each speech upon the paper, the sequence of the same, and all details which would present themselves to his physical eye. Thus he would acquire the *words*, not always perhaps the sense, as he would have no time to think about the context; and in some cases if a wrong cue were given it would probably tend to throw the student off the track and upset him altogether. I have known many ludicrous incidents occur as a result of such misadventures. In my early days I had to do a great deal of rapid study and cramming, and soon after I began to play leading business I studied and played six long parts in one week with only two of which I was at all familiar. I once took Sir John Falstaff at twelve o'clock and played the part perfectly at night. This, I think, was the most terrible strain I ever had. I need not say to you that I could not get at the meaning of the character under such circumstances. I also, once, for a wager, studied and repeated perfectly a difficult speech of 17 lines which was read to me slowly 12 times. I give you these as matters of my own personal experience as being those I can myself vouch for. I am sure that nearly all actors of position can tell you much of the same kind. One result of these hurried studies is the words do not remain. They seem to fade from the memory as rapidly as they were acquired. I am quite certain that this process is purely mechanical, and that it is not possessed by all. Men, as a rule, study more quickly than women; in fact I have met very few women who could 'wing' a part. On the other hand I think women are more accurate than men and not so apt to substitute their own words occasionally for those of the author.

If any other points occur to you do not hesitate to write me, and I will help you all I can.

Faithfully yours,

HY. EDWARDS.

Prof. Osborn.

A Correction.

In my discussion in the last issue of the Review, pp. 68–69, I made the mistake, in quoting from Professor Small's article in the *American Journal of Sociology*, of attributing to him the word 'poach,' inadvertently taking it from a private letter from him on the same subject. As soon as I discovered this, I expressed my great regret to Professor Small, and he tells me that he meant nothing by this word beyond what he had said in print in the article referred to. While therefore, as he is kind enough to say, the mistake is 'immaterial,' I myself much regret it, and take this means of publicly saying so.

J. Mark Baldwin.

PSYCHOLOGICAL LITERATURE.

Dictionary of Philosophy and Psychology. Written by many hands and edited by JAMES MARK BALDWIN, with the coöperation and assistance of an International Board of Consulting Editors. The Macmillan Company. Vol. I., pp. xxiv + 644. 8vo. Vols. II. and III. in preparation. $5.00 *net*, per vol.

It is certainly a notable event in the world of science and letters when representative contributors to philosophy, psychology and related sciences, unite in the preparation of a dictionary of the terms employed in the philosophical disciplines. One cannot fail to see in this undertaking a significant phase of the movement towards scientific collaboration which is showing itself as a corrective of specialization in every department of investigation. And it is equally clear that such a collaborated dictionary may be expected to do much towards eliminating the personal element which has always played too large a part in the use of philosophical terminology, and that it will hasten the day when the ideal of universal, objective validity shall be attained for the conclusions and principles of the philosophical sciences. The conception of such a dictionary was indeed timely, and its preparation has put scientific students of all departments, especially students of the departments most intimately involved, under very large obligations.

The complete dictionary is to consist of two large-octavo volumes of definitions and articles, and an additional volume of bibliography. The first volume, with the general introduction and with terms as far as 'Law,' appeared in September of last year. The other volumes are promised to appear soon. The first volume gives an insight into the mode of preparation, and also gives a fair basis for at least a preliminary judgment of the excellence of the whole dictionary. It will, accordingly, be proper to anticipate the later volumes by a general description of the character and scope of the work, and some estimate of the degree in which it has thus far attained the aim which it has set for itself.

The editor invited to his aid a corps of associates and contributors numbering sixty-three in all, and including many of the most eminent scholars in America and England. There were also representative committees of foreign scientists who contributed suggestions and criticisms on French, German, and Italian equivalents, which equiva-

lents, given with each of the terms of the dictionary, constitute one of the important features of the work. The editor has very properly pointed out in his preface that it is entirely justifiable to enforce the decisions of such a body of authorities even when the enforcement of their decision means the sacrifice of personal usages. And certainly students will look eagerly for the decisions of this 'International Committee' on the knotty problems which they have taken up.

With reference to the qualifications of the editor for a work of this kind there can be but one verdict. Professor Baldwin's writings have shown him to be an investigator of high rank, and a contributor to many phases of psychology. The biological sciences on the one side, and the ethical and social sciences on the other, are all treated by him in his works on genetic and social psychology. His broad interests and preparation for the work of editing the dictionary are fully attested by the scope of his contributions to the body of the text. As an original contributor, or as a critical emendator of the contributions of others, his signature appears on the majority of the articles, and his earlier writings are constantly drawn upon as illustrations of standard usage.

The compass of the dictionary may be described by saying, first, that it deals chiefly with subjects belonging to the philosophical sciences and disciplines and with certain closely related topics in neurology and biology. In the second place, such subjects as are discussed, are taken up because of their present-day interest and utility. The historical treatment of philosophy has been largely passed over, except in those cases in which reference to history would contribute to the elucidation of the present use of terms. This attitude towards historical study is emphasized by the editor's prefatory warning to students of Greek and Mediæval philosophy that they will find these subjects only partially treated.

The terms selected for discussion certainly include the terms promised by the principles of selection just described. Serious criticism cannot be directed against the dictionary on the ground of omissions. On the contrary, one cannot help feeling that there has been a too generous departure from the true field of such a work, and an expenditure of valuable energy and space on certain irrevelant terms. Thus, take the following list of articles of somewhat the same general character under the letter A:—Admiralty Jurisdiction, Apprenticeship, Arbitrage, and Arbitration. Or again, consider the fact that a very large number of theological and eccleciastical terms such as Anabaptists, Adiaphoristic Controversy, Atonement, and so on, have been

introduced. It is not that the work on these terms has not been admirably done, for it is of a uniformly high order, but it is a serious question whether it pays to duplicate work on theology, which has recently been very ably and more comprehensively done elsewhere, in a dictionary of present-day philosophical and psychological terminology. And the question rises to the dignity of a criticism of inconsistency of plan when one recalls that mediæval philosophy has been consciously reduced to a minimum.

One department which seems entirely below the level of the rest of the work is the department of biography. This department, in the first place, does not contain the name of any living writer. It is therefore purely an historical record, and as such, contributes very often little or nothing to the main currents of discussion in the book. But even judged by itself as an historical record it must be criticised as very irregular and as very incomplete. Minor names often receive quite as much discussion as those of large importance. The matter furnished under each name is indeed, as the editor remarks in his preface, meager. It might have been made useful even though brief, if it had contained references to sources of fuller information. Furthermore, the list of names selected illustrates in a curious way the capriciousness of the Goddess of Fame. One is tempted to believe that somebody's life of the saints and church fathers must have been very accessible when the selection of immortals was made. Then, too, certain American and English professors of theology who must base their largest claim to a lasting world-wide reputation upon the fact that their names appear here, are given five or six lines in which to record their various pastorates, while Friedrich August Carus, the historian of psychology at the beginning of the last century, and Galileo and Bolingbroke and others go unmentioned.

As to the general articles, many of them are brief, consisting of a definition, a summary of the discussions which center about the term, and a general bibliography. Such short articles may be regarded as dictionary articles proper. Then there are more elaborate articles of an encyclopedic type, some of which treat of terminology, some of which deal with topics considered to be of more general importance. Thus, in this first volume there are several long articles on terminology by Professor Royce, treating of the following special topics: Greek terminology (8 pages), Hegel's terminology (11 pages), Kant's terminology (9½ pages), Latin and scholastic terminology (11 pages). Other encyclopedic articles of notable length are on the subjects: Brain (21 pages), by President C. L. Herrick and Professor C. J.

Herrick; Laboratory and Apparatus (11 pages), chiefly by Professors Warren and Titchener; Hearing (9 pages), by a number of writers; Language (8 pages), by President Wheeler; Beauty and the Beautiful (5 pages), by Professor Tufts; Ethics and Ethical Theories (5 pages), by Professors Sidgwick and Sorley. Other shorter encyclopedic articles of this same general type are present in abundance, but cannot receive individual mention.

These encyclopedic articles make it possible to deal easily with a great many single terms. The terms are defined in their proper places in the longer discussions and are repeated with references to the general article in their proper alphabetical order. This is perhaps the simplest way of solving a difficult problem, but it detracts very much from the utility of the book as a dictionary. The reader who turns to Chronoscope, to take a chance illustration, and finds himself referred to an eleven-page article on Laboratory and Apparatus, will probably feel like suggesting that the eleven-page article be at least interspersed with heavy-faced type indicating the various subtopics which are treated, or that it be indexed at the end as are the articles on terminology. And he will probably fail to see why chronoscope should be classified under laboratory any more than under its own proper initial.

If now we turn to a critical study of the articles themselves, we are impressed with the rich fund of philosophical and scientific knowledge which has here been brought together. Many of the articles are distinct contributions to thought because of the lucidity with which they have arranged the available data bearing on the topic in hand. It is wholly impossible to do individual justice to the many excellent contributions in the dictionary. Indeed, towards many of the articles the present reviewer must frankly take the attitude of a student and express merely a student's appreciation of the comprehensiveness and clearness of their treatment.

Without attempting, then, to give a critical analysis of all parts of the work, the remainder of this review will be devoted especially to the psychology of the dictionary.

Many of the articles on Psychology are written by the editor and Professor Stout, the two contributors often working together. Professor Jastrow has contributed the larger part of the matter on abnormal psychology. Professors Titchener, Warren, Cattell and Sanford, and Mrs. Franklin have written on experimental subjects; and one or two other writers are occasionally called upon.

One is disappointed that there has been so little coöperation among these contributors. They have worked for the most part singly or at

most in twos. It is almost humorous to see the whole company brought out together just once, and their opinions carefully chronicled as to whether the term 'clang' shall or shall not be used, while there are a great many other recommendations of sufficient importance, one would think, to require joint consideration if authority is to stand for anything, which recommendations have very limited support, at least in the number of those who appear as sponsors. Thus, for example, a whole series of terms for volition is suggested under the term action, which series is (to say nothing of its incompatibility with the scheme of German equivalents under the term conation) of doubtful utility. 'Involuntary,' as defined under action and in a later independent article, is certainly a radical departure from the best present usage. One cannot refrain from asking in such a case, where were the other councilors? Or take such terms as Affect, Affection, Colligation and Idea, under which recommendations are made in direct criticism of recognized usages. Such recommendations should be sanctioned by sufficient authority to raise them above the level of mere polemic, or else they should be omitted. It is to be recognized, of course, that this lack of coöperation does not detract from the excellence of the content of many of the articles. But the only way in which the work can be brought at all points to a uniform level is through patient joint consideration.

It is interesting to raise the question whether the individual factor has been successfully eliminated from the various discussions in the absence of the complete joint criticism which would seem to have been desirable. And the answer will differ for the different topics. Thus, 'depth' and 'fatigue' furnish perhaps as good illustrations as any, of topics which have received impersonal consideration. But these were fairly impersonal from the outset. Articles on 'Laura Bridgman' and on 'Illusions of Motion' are encyclopedic articles, and would seem to indicate that the interest of author and editor in these special topics had overbalanced the dictionary idea and allowed a greater latitude. But more important than personal decisions on matters of scope are certain other inconsistencies and limitations in use and definition which make it impossible for one to accept as absolutely final and universal the recommendations of the individual writers.

A striking case of this latter type will be brought out if one investigates the very fundamental question, what does the dictionary have to say on the nature of the elements of consciousness? An element of consciousness is defined as 'any content of consciousness in which introspection fails to detect internal complexity.' We are warned,

however, that "this use of element is opposed to that which applies the term to the original mental functions from the point of view of psychological analysis. The element is such for psychical or mental analysis." No enumeration or illustration of what are currently accepted as elements of consciousness is here undertaken. There are, however, two references relating to the negative part of the definition so that we may, by looking up 'psychological analysis' and 'classification of functions,' find out what are not, according to the dictionary, properly to be designated as elements of consciousness. And there is one reference on the positive side to 'psychical analysis.'

Taking up the negative references, we find under psychological analysis the following: "Analysis * * * in psychology consists in the reduction of complex states of mind to the simpler elements or factors which compose them." Evidently there is confusion ahead when we are forbidden in one article to use 'elements of consciousness' for the results of psychological analysis and are in another article furnished with another kind of 'elements or factors' as the result of such analysis. But let us continue with the negative definition of elements of consciousness. Under 'classification of mental functions' we find the following mentioned as being regarded by various writers as 'ultimate functions': 'intellection,' 'conation,' 'cognition,' 'feeling,' 'will' as a synonym of conation, 'affective consciousness' as an explanatory synonym for feeling, and finally Brentano's 'judgment or belief.' The special term 'classification' under which these are mentioned is defined as 'Distinction of the fundamental constituents of every concrete state of consciousness.' These 'fundamental constituents' or 'ultimate mental functions' are, it must be remembered, not included in the definition of 'elements of consciousness,' for that definition clearly opposed itself to the use which applied the term element of consciousness to the 'original mental functions from the point of view of psychological analysis.'

Turn now to Conation and one finds it defined as 'the theoretical active element of consciousness, showing itself in tendencies, impulses, desires, and acts of volition.' To say nothing of the difficulty of supporting this as a generally acceptable definition of conation, we have still the difficulty, from which even the adjective theoretical does not save us, of squaring this usage with the definition given under 'element of consciousness.'

Or turn to 'affection' and we find that it is 'the hypothetical elementary form of feeling.' Or again, under feeling, we have the recommendation "(1) that Feeling be used for this phase of experience

in its combination, with knowledge and will, in a concrete state of mind, *i. e.*, as a consciously made abstraction from a richer whole; (2) that Affective be employed as synonymous with feeling used adjectively; and (3) that Affection be used for the purely hypothetical element which underlies the concrete manifestations of feeling." Or to use the phraseology of a few lines earlier "the noun affection is applied to the abstraction itself considered as a hypothetical element in the mental life."

Finally, cognition is 'the being aware of an object' and "as above defined, cognition is an ultimate mode of consciousness coordinate with conation and affection."

This lengthy study of the teachings of the dictionary on at least one fundamental question justifies two conclusions. The first is that it is exceedingly difficult to give general definitions of certain widely used terms such as 'element of consciousness'; and the secondis that one's own personal psychology, whether it be in favor of the threefold classification or some other classification, will ultimately determine the way in which he will use terms and define them, and will probably appear sooner or later in strong categorical definitions. It may be that the present reviewer fails to comprehend the insurmountable difficulties in the way of greater coöperation and wider criticism from the whole body of consulting psychologists, but it would seem natural to suggest that greater breadth could have been given to some of the definitions by finding out, first, what is not accepted as universally valid, and then by elaborating at these particular points until the definitions were sufficiently comprehensive to cover at least all the usages in the dictionary itself. As they stand 'element of consciousness' and 'conation,' at least, would hardly be acceptable to such writers as Wundt, Münsterberg, Külpe, Titchener and others, even if we overlook the internal difficulties in the dictionary itself.

The desire to see greater elaboration of some of the psychological articles becomes still more pronounced when one turns to the long treatise on 'Brain,' or to the lengthy discussion of 'Galton's Law,' or to some of the other articles such as the one on 'Laboratory and Apparatus,' which really involve no serious difficulties of terminology. The articles mentioned are excellent in themselves, but they seem somewhat too comprehensive to fit into the general plan.

But criticism must not be wholly, or even in the main adverse. The editor has anticipated some objections to the way in which the work has been carried out and has reminded the critic that "he is one, we are many." And certainly it would be an egregious fault of

criticism to magnify the objections to the extent of overshadowing the virtues. Universal and final no dictionary can be. With a great body of writers contributing to psychology, and some of the best of them 'erratic,' as the dictionary puts it, in the use of their terminology, it is certainly a great service to science that a beginning should have been made in the preparation of concise definitions. The dictionary will undoubtedly give rise to many discussions, but these discussions will be more careful in the matter of terminology than such discussions have ever been before. Translators who have struggled alone with the problem of finding English equivalents, or foreign writers who have been at a loss to understand some of our terms, will also be very greatly aided by the equivalents in four languages which the dictionary has selected so carefully and so satisfactorily in most cases.

The second volume will be awaited with great interest by those who have studied the first. It is surprising to find how many important psychological terms belong under the last fifteen letters of the alphabet. One is constantly reminded that he has only the first half of a comprehensive work in his hands by the cross references which so often carry him to the second volume. The editor and his associates undertook a large task in the preparation of this work. Their results are certainly quite in keeping with the comprehensiveness and the boldness of the undertaking.

Charles H. Judd.

University of Cincinnati.

An Introduction to Psychology. Mary Whiton Calkins, Professor of Philosophy and Psychology in Wellesley College. New York, The Macmillan Company. 1901. Pp. xv + 511. With Bibliography and Index.

A few words will suffice to make plain the general plan of this book. The work is divided into two parts and an appendix. Book I. comprehends a treatment of the normal human mind and its laws; it is complete in itself, and to its scope the discussion of mind has commonly been limited in contemporary manuals. Book II., virtually an appendix to the first, is concerned with two subsidiary topics, the nature of mental activity in simpler forms of life (Part I.: 'Comparative Psychology') in which the processes of brutes and children are discussed; and the description of transient and permanent disturbances of mental function in the human being (Part II.: 'Abnormal Psychology'). The main body of the work is divided into two parts, in the first of which the structural elements of consciousness are ana-

lyzed, while in the second the concrete mental state is considered with especial reference to its significance as a personal attitude. Upon the importance of this methodological division the author lays stress. The contents of the first part will be readily surmised in so far as the nature of such elements is matter of common agreement. An enumeration of the special sense elements is followed by a description of those connected with internal excitation and the consciousness of motion. The question of primitive extensity as an elemental consciousness is next taken up, and upon that succeeds the discussion of a variety of factors which are not uniformly regarded as elementary. These are the attributives: the affections and feelings (why 'feelings' and not *feeling?*) of realness, and the consciousness of relation. It will perhaps somewhat surprise the reader, after learning the method of division upon which the book proceeds, to find that the discussion of Attention brings this first part to a close, instead of introducing the consideration of concrete consciousness experiences. In the second part there is taken up a variety of processes, commonly treated as separable formal functions, in the following order: Fusion and Association, Perception, Imagination, Generalization, Judgment and Recognition; next a group of topics which by common consent are representative of concrete experience, namely emotional moods, with attitudes of will and faith; and finally the typical personal relations and the social and religious consciousness. The book concludes with a *résumé* of important historical concepts in the domain of psychological inquiry.

If there were need to disarm criticism, the writer of this book has made a valid plea in her prefatory statement that it is the embodiment of a course of lectures given to the elementary class in psychology at Wellesley College and is printed primarily for their convenience. Every such work, nevertheless, is inevitably more than a local handbook. It appeals to the practical needs of the whole body of professional teachers, to the theoretical interests of the psychological student, and to a rapidly increasing body of readers whose interest is human rather than technical, who are interested in the problems of psychology as they are in the discoveries of science, in historical research and in literary expression. The excellences looked for in such a work vary with these types of interest, and their comments are not necessarily consonant.

The prime intention of the writer is to produce a satisfactory college text-book. Such a work calls for lucidity of treatment, at large no less than in detail, accuracy of statement, and readableness. In

these regards the present book is one of high merit. It gives a distinct impression of form; no part is unduly expanded, as might readily be done either on account of the richness of illustrative literature which has accumulated unevenly about special topics, or in consequence of personal interest in particular problems. Throughout the book one feels the unity of the whole method of treatment. Points of historical value or importance in connection with the advance of scientific method, but which are of minor rank as aspects of significant human activity, are distinctly subordinated, or treated in an appendix.

The two other points may be mentioned together. The author's treatment of visual sensation will illustrate both aspects in a single instance. The skill with which the details of an intricate subject-matter are here subordinated to a clear exposition of important facts seems to me to be characteristic of the whole work, and I know not any part of it where the student may reasonably fail to follow the thought of the writer. These points are of especial importance to those readers whose study is not assisted by explanatory lectures, and they will appreciate the rare skill with which the author has translated abstract terms and concepts into concrete images, and brought the formal discussion of mental functions into touch at every point with the literary expression of experience.

Matters of personal acceptance or dissent on the part of the reviewer in regard to questions of classification and method are of secondary interest to those general characters of excellence of arrangement and exposition which appeal to every reader regardless of his relation to the theoretical positions assumed by the writer. Points of especial importance, however, may be named in the discussion of the physiological bases of affective qualities, of the forms of association, of the range of structural elements of consciousness, and of judgment and reasoning. Points of interest in relation to the personal standpoint of the writer are presented in the discussion of sensational extensity, the differentiation of perception and imagination upon the basis of immediate social reference, the non-sensational feelings, and the methodological division of the subject-matter into facts and attitudes. Points of more or less important dissent on the part of the reviewer appear in the numerical analysis of sense qualities, especially in reference to the terms of colorless light and of temperature sensations, in the theory of mixed emotions, in regard to the immediacy of the social reference in perception, and in the justification of the separate treatment of concrete personal experience in a scientific text-book.

The book is enriched by a well-chosen and sufficiently comprehen-

sive bibliography. The value of such a literary index to the student beginning his study lies in the definiteness with which he is directed to sources of information in regard to particular points. To be of the highest service the compiler must do the sifting quantitatively as well as qualitatively, and the present list is to be commended for the care which has been shown in the indication of literary sources under each topic considered in the book. The index with which the work is furnished is a topical one instead of being, as many subject-indexes are, exasperatingly verbal in its references. The book is illustrated with thirty-two tables and diagrams and thirteen figures in the text. The publishers are to be complimented on a handsome piece of book-making.

ROBERT MACDOUGALL.

Phaenomenologie des Wollens. By DR. ALEXANDER PFAENDER. Leipzig, Johann Ambrosius Barth. 1900. Pp. 132. Crowned with the Frohschammer Prize by the Philosophical Faculty of the University of Munich in December, 1899.

In the monograph before us the author says he will be scientific, beginning with facts and proceeding to common notions and laws. His method is strictly psychological, retrospective rather than introspective. He does not try to show the causal basis in which the will is grounded, but rather to take to pieces the actual facts of the consciousness of willing. A wider and a narrower use of the word will is made, viz., as effort in general (Streben), and as volition. The latter being a special form of the former, the end of volition is always an object of effort, but an object of effort is not always a voluntary aim. The idea of some experience toward which an attitude of longing, based on appetite, is felt is the essential part of effort. This feeling of longing or tendency-toward is elementary and irreducible, but it is like pleasantness and unpleasantness in its subjective character, all three being modifications of self-feeling (des Ichgefühls). This particular elementary feeling is called, throughout the work, the feeling of effort or effort-feeling. What are the marks of the thing striven for, and how does it come that effort-feeling is joined to this one of all the ideas that enter into the mind?

This is the problem of the relation of the ego to the objective content of consciousness. Upon this relation of the total content of consciousness to a single ego depends the unity of consciousness. Different elements differ in the closeness of their relation to the ego; one is spoken of as clearer, stronger, more vivid, as attended to, etc., to distinguish it from less prominent elements. We should distinguish be-

tween the activity called attention and the consciousness of the activity; the latter consists in effort-feeling, but the more the desire to attend succeeds and the closer the relation of the ego to its content becomes, the more the effort-feeling vanishes. "The relation of self-consciousness to an objective content consists in a unity of ego and content which cannot be further defined" (19). This relation of attention can fluctuate in the degree of its closeness; the extent of that attended to may be larger or smaller; one may attend to a whole as a whole or as a combination of parts; and the part attended to may be now one and now another. In the consciousness of willing or effort the idea of something striven for is present, but the idea (as we think of it) is only the representative of that which is striven for, not the thing itself. The idea is the object of attention; the thing, the object of effort, toward this we experience the feeling of effort. But now the question arises, in what does this intention, or direction toward something not present but represented by an idea, consist?

Popularly it is thought that the idea of an end is an idea filled with pleasure, but this is really not the same as being an object of effort. The idea of pleasure need not be present at all, and it is idle to say that some idea of pleasure is the unconscious end of volition; or, that as a result of habit the pleasure that was once the end-aim has dropped out of notice and the means to its realization become the only end of which we are conscious; or that what is sought is always relatively less unpleasantness or more pleasantness. The important thing is the *present*, and not the represented, pleasure-pain effect of the represented experience, the relatively pleasant feeling which the represented end awakens at the present moment. The feeling of relative pleasure does not arise through a conscious comparison of present feelings, for this is impossible, but it is an increasing pleasantness or decreasing unpleasantness or transition from unpleasantness to pleasantness with the anticipation of the desired experience (57).

Wherein does the feeling of effort consist? Not in a feeling of relatively greater pleasantness or relatively less unpleasantness. All of these may be present in consciousness together—relative pain, relative pleasure, and effort-feeling. The feeling of effort is the immediate experience which gives to the conception of effort in general its specific meaning, and this feeling is a qualitative or attributive element in the consciousness of self.

Positive and negative effort may be regarded as two modifications of the feeling of effort. In positive effort the ego feels itself one with the object of its effort, and in negative, as different from its object.

In the same way, in pleasure we feel one with the object of the pleasure, and in unpleasant experiences we feel different from the object of the unpleasantness. Hence the general feeling of effort may become an effort for something by the addition of pleasure, and a struggle against something by the addition of pain or unpleasantness. The former, however, is accompanied by an increase in the exclusiveness and inwardness of the struggle-tinged attitude of regard, and the latter is accompanied by a decrease in these feelings. There are instances where the idea of the existence of an object of effort, in comparison with the idea of its non-existence, gives pain, but this is where the chosen end is the less of two evils.

The object of positive effort may be either the represented event itself, one's own part in the realization of the event, or the heightening of the worth of one's own person. When one chooses the unpleasant alternative, it is not this which constitutes the true object of his endeavor, but the exaltation of his own personal worth; and this idea gives him a present pleasure. One may strive to possess capabilities upon which he lays no great stress, because they are highly prized by others and because one may exalt his own worth *in the eyes of others* by acquiring them. Under pleasure and pain are thus to be included every sort of satisfaction and dissatisfaction. Understanding the terms in this broad sense, we may say there is no instance of striving for the attainment of an object or of striving to prevent the occurrence of an event, in which the anticipation of the end does not color one's effort, in the one case, with pleasure, and in the other, with pain; and this is true even when the anticipated end involves an anticipation of pain. The author inclines to the view that this addition of pleasure or pain to the feeling of effort makes the latter, in the one case, effort-toward, and in the other, effort-away-from. To decide the matter we should see whether the antithesis of for-and-against does not occur in other psychic functions, as (*e. g.*) in theoretic affirmation and denial, and whether it occurs there with or without relative pleasure and displeasure at increasing inwardness of view.

In the second part of the work the author treats as a special form of effort, volition, the first question being what modification of effort constitutes volition? At the close of this part the results are gathered together as follows. In order that we may have volition in the proper sense of the word, "there must be added to the fact of effort (1) a belief in the possibility of realizing the object through one's own actions; (2) the feeling of effort, which thereby acquires the character of power, must extend itself so as to cover the conditions of the

realization of the object (the effort must become an effort for the realization of the object); (3) in this extension so as to cover both the conditions and the consequences, the effort-feeling must retain the characteristic of (relative) freedom (the effort toward the end must remain victorious), and (4) the feeling of effort must retain or acquire the characteristic of a predominating spontaneity: then and then only will the effort have become a volition in the proper sense of the word (131, 132). A feeling of positive effort possessing the marks of power, freedom and spontaneity are the essentials of volition, and the feeling of power must relate both to the end of volition and to the means to its realization."

We may now will a future action or event, or we may will that a certain action or event now take place; and in the former case any interval of time less than a life may elapse between the volition and its fulfillment. In neither case can the end of effort be its cause, because the end of effort does not exist before the effort has taken place. Motives are causes of effort, although not all such causes enter into motives. Every impulse and effort has a cause, but not every one has a motive. Motive cannot be identified with the end-aim of volition; motive is the whence and end-aim, the whither, of volition. A motive is not the end striven for, but the representation of this end, together with the impulse or tendency toward it. Motives are not something outside the ego, and determination by a motive is self-determination.

It really means nothing to inquire after the motive of any action which is not the means to the realization of some end-aim. Striving for an end-aim is the motive for the effort to realize the conditions upon which the realization of the end-aim depends; but the total fact of volition is itself not motived. "All proper volition stands outside of motivation, is for consciousness something final and, in this sense, free. It is not denied that every volition has its psychological causes; only, one must not call these causes motives." The question as to the motives of volition always refers to the volition of deduced ends which are related to an end-aim as means.

This discussion is written from a psychological point of view, the method consisting largely in retrospective analysis and definition; and, within the limits of this task and the necessary limitations of brevity, it has been admirably done, or so, at least, it seemed to the reviewer. Among points of excellence possessed by the discussion clearness is very prominent. The distinction between the present pleasure of the representative idea and the expected pleasure or gain of

the end striven for is consistently maintained. The thought that the psychological value of an end depends upon the closeness of the relation felt between the ego and the end, a relation involving a significant social reference, is a very important one. The discussion might have gained somewhat had the difference between volition, as an objective phenomenon, and willing or self-determination been introduced.

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ETHICS AND RELIGION.

La Morale ancienne et la Morale moderne. V. BROCHARD. Revue Philosophique, January, 1901, pp. 1–12.

M. Brochard's thesis is that noteworthy differences of conception in historical systems may well help us in properly grasping and attacking difficult problems. As instances of such differences he points out that the Greeks and Romans conceived of God as limited and as pure or passive intelligence, while modern philosophers conceive of him as infinite and as omnipotent will; that, while the ancients thought of matter as different from body, the latter requiring form as well as matter for its full specification, in modern thought matter is identified with body; and again that, while matter is the vague, the indefinite, the unspecified to the Greeks and Romans, to us matter is, if not identical with extension, as Descartes taught, at least, essentially extended and figured.

But the chief differences M. Brochard points out and discusses are those distinguishing the moral conceptions of the ancients from our own. Ancient philosophers had no conceptions of, they even had no words for, duty, moral imperative, conscience, sin, responsibility, or moral liberty, which find so important a place in our moral systems, often lying at their foundation, and though the conception of immortality was familiar to them, it had for them no ethical import.

To be sure duty, and the moral conceptions allied to it, were present in the religious thought of the Greeks and Romans, and even of earlier peoples. But they remained religious conceptions, and no account was taken of them by ethical philosophers. As a religious phenomenon morality was essentially a matter of duty, as an ethical phenomenon it was essentially a matter of conduct profitable to the individual. To us, on the other hand, both the problem of duty and the problem of self-interest are ethical problems.

On this basis, M. Brochard makes the suggestion, very tentatively, that the ancients may have been nearer right than we. He would not

be surprised if the confusion in our ethics, and the failure to establish the science on a positive foundation, were due to the futile attempt to combine two inconsistent points of view.

In support of his position he might have cited the difficulty experienced by Butler, by Sidgwick and Utilitarians generally, and even by Kant, in reconciling duty and self-interest.

But in criticism of his suggestion it should be urged that obligation and the rest are facts, and basal facts, of the moral life, that their relation to religion is quite as much one of support as one of derivation, and that they are products and constituents of the social consciousness as a whole, which includes the religious consciousness, but includes much more besides. S. E. MEZES.

UNIVERSITY OF TEXAS.

Les principes de la morale; La loi morale. CH. DUNAN. Revue Philosophique, June, 1901, pp. 594–624.

This is one of a series of three articles in the *Revue* under the same caption, dealing respectively with the highest good, conscience and the moral law. The three are, moreover, extracts from the author's forthcoming book, 'La Morale,' which is to form the third part of his 'Essais de Philosophie générale,' the two earlier parts of which dealt respectively with psychology, empirical and rational, and with metaphysics.

Naturally, conceptions and points of view whose elucidation must be sought elsewhere, appear in this article. But, on the other hand, the author's views have a cosmic sweep that is satisfying, they are articulated into a unitary whole under a watchful eye, and their exposition gives opportunity for the exercise of the high order of keenness and dialectical skill which readers of M. Dunan's earlier writings have come to expect of him. To be sure, his treatment is somewhat abstract, and his dialectics at times border close upon, if they do not verge into sophistry, but the general soundness of his conclusions and the free use of the facts of biology and of the history of ethical theory remove all grounds of serious quarrel. M. Dunan's ethical system seems to be a synthesis of Kant, Spencer and the Stoics unified by his metaphysic.

In the first article morality is said to consist, as the Stoic formula runs, in 'life in accord with oneself and with universal nature,' and that, too, is declared to constitute the *summum bonum* for each man. In the second article the author argues that, just as what he calls the vital instinct sees to the preservation of life in sub-human beings, so

does conscience see to the preservation of man's life as such, *i. e.*, as a self-conscious, rational and self-controlled life; a position that, allowing for pathological cases, is no doubt sound enough although little evidence is given in its support, but always with the proviso that different consciences preserve very different degrees of rationality and self-control, some consciences being all but truncated into blind impulsions, whose support of rationality is wholly unconscious and very imperfect. The rational life preserved by conscience is thought to be a life of self-accord as well, inasmuch as it is irrational to disregard the claims of any vital function, and of accord with universal nature, because, without at least a minimum of accord with the whole of things, self-accord and rationality could not be maintained.

Coming to the third article, which is the special object of this notice, M. Dunan maintains that the law of duty is in fact a categorical imperative; that it is rightly categorical, because duty is rationality in man commanding reasonableness of conduct, and for such a command no further reason could be given without disloyalty to rationality as the supreme end; and that it must be categorical, because living beings, according to his metaphysics the only beings that exist, are necessary beings, since non-being is contradictory, and therefore must will the necessary condition of life and being—which in the case of man is conduct that follows the law of duty—and will it categorically. As to the first statement, it seems to be true as long as conscience is a blind intuition, and even when it reaches the Stoic, Kantian, or Puritan stage that prescribes rigid and abstract laws of duty. But when conscience develops into a fully rational faculty, that is judicially considerate of all pertinent interests, its commands are not infrequently halting, checked by the 'if' of modest ignorance. The second statement would seem to be justified for the reason given, and because in so far as conscience champions fully rational conduct, its self-sufficiency is not arbitrary, since conduct is rational only in so far as it considers *all* pertinent conditions, and to that extent there are none left to condition it. But the third argument seems more than doubtful. Even granting the metaphysical position, admitting that being in general is necessary, does it follow that any particular being, *e. g.*, man, exists necessarily? Would it be maintained that the disappearance of the human race would involve a contradiction?

While accepting duty as a categorical imperative with Kant, M. Dunan is anxious to give it a concrete content, and discusses the question at some length. The author's main point is that duty, in commanding the greatest possible active accord with oneself and with

universal nature, in effect commands life at its fullest, richest, and deepest. Moreover, what duty commands is human life, which, as such, is to be in harmony with all other lives, and with the organized totality of living beings that forms the unitary living whole properly to be called God.

Interesting, too, is the author's reconciliation of duty and happiness. Admitting apparently that any definition of morality making it inconsistent with happiness would be mistaken, he goes on to argue that a life in accord with self and with universal nature is the necessary and sufficient condition of happiness, and that, therefore, while happiness is not the supreme end, in seeking the latter happiness is best secured. But the pursuit of pleasure M. Dunan condemns severely. It necessitates the discordant exercise of some few functions to the neglect of others.

For the rest, in opposition to Kant the author maintains that the moral law is not everywhere the same and unchangeable. In agreement with Kant he argues with much subtlety of dialectic that it is a law of autonomy. In further agreement with Kant he holds that the following of genuine good intention insures absolute morality for the agent in each case, with the warning, however, that practices condemned by all civilizations, even the grossest, 'like perjury and assassination, * * * which cannot be admitted by any form of organization of human life,' cannot be approved 'in good faith' by any agent. To Kant's separation out of will, and exclusion from it, of all sentiment and feeling, M. Dunan dissents vigorously and convincingly, emphasizing especially Kant's inconsistency in admitting within good will the sentiment of respect. The article concludes with a discussion of the relation of the author's views to those of the Stoics, Aristotle, Kant and Spinoza.

This brief notice can give but an inadequate idea of the grasp and precision of the author's thought, of the serried closeness of his reasoning, and of his argumentative resourcefulness. To gain such an idea the three articles must be read.

S. E. MEZES.

A Psychological Test of Virtue. By GEORGE M. STRATTON. International Journal of Ethics, XI., January, 1901. Pp. 200–213.

The subject of this discussion is Professor Dewey's theory of conduct, as represented in his syllabus, 'The Study of Ethics.' In Professor Dewey's view action in its earlier stages is impul-

sive, the impression of the moment calling forth an immediate response; in the later stages (represented in relatively reflective human action) the simple impulse has to deal with competing impulses, and action is the result of their interplay. The characteristic of the more developed person, then, is that each of his acts is participated in by a wider variety of activities. As such it shows the various sides of his character, the hasty, superficial, impulsive reaction telling of but a single side. In this Professor Dewey discovers the basis for eternining the moral value of what we do; an act is morally good in so far as it represents a pertect coördination of all sides of our character; so far as it represents only one side of our character it is wrong. Now what our author criticises is not Professor Dewey's doctrine itself but its claims to represent the standpoint of psychology. In Professor Dewey's view the real man is represented only in a perfect coördination of all his impulses; but for psychology all the activities of the man, many-sided or one-sided, mutually consistent or inconsistent, are equally expressive of his real nature. Professor Dewey's standpoint is, accordingly, the standpoint not of psychology, but of the metaphysical doctrine of absolute idealism. As a result of this standpoint he ignores the difficulties of the ethical problem. Assuming that the direction of coördination is somehow exhibited in the empirical character of the impulses themselves he fails to state in concrete terms how the ideal coördination is to be effected or in what it consists. Accordingly, he fails to show the connection between the real self, represented in perfect coördination, and the imperfect self, represented in the empirical conflict of impulses. Thus, by leaving evil actions essentially unconnected with the real person, he takes away all moral responsibility for such conduct.

WARNER FITE.

L'évolutionnisme en morale, étude sur la philosophie de Herbert Spencer. JEAN HALLEUX. Paris, Felix Alcan. 1901. Pp. 228.

As announced in his title the author takes Herbert Spencer as his type of evolutionary moralist. His treatment of evolutionary ethics reduces itself, therefore, to an analysis and criticism of the evolutionary hedonism presented in the 'Data of Ethics.' His analysis of the Spencerian theory, which occupies the first part of the book, is reasonably clear and objective. His criticism proceeds apparently from the standpoint of the Catholic theology and does no more than repeat the argument with which the evolutionary conception was greeted at its

first announcement by theologians generally. He holds that human life cannot be conceived as a further development of animal life; and that the course of history does not point to the future condition of universal human happiness which hedonism expects. Mr. Spencer's system fails, moreover, as an ethical theory, to satisfy our moral consciousness and to explain our sense of duty. And, finally, from a practical standpoint, it offers no sufficient motive for moral effort. The author makes no attempt toward a systematic formulation of his own view, but from occasional remarks it appears that his philosophical system (which is also a system of theology) is practically identical with the idealistic view known in ethics as the theory of self-realization.

WARNER FITE.

UNIVERSITY OF CHICAGO.

Les Maladies du Sentiment Religieux. E. MURISIER, Professeur à la Faculté des Lettres de l'Académie de Neuchâtel. Paris, Félix Alcan. 1901. Pp. 175.

This is not a clinical study of religious maladies, but an essay on the nature of religion. It is based, however, upon analysis of certain religious abnormalities under the theory that, just as the progress of dementia reveals the nature of the mind by destroying its functions one after another in the inverse order of their evolution, so the self-destruction of religion in its extreme forms may be made to reveal the essential nature of the religious impulse. Two such extreme forms, ecstasy and fanaticism, occupy substantially the whole of the author's attention. These are studied almost altogether at second hand, that is, through biographical and historical literature.

The main thought of the essay is the reduction of the contemplative and active types of religion respectively to egoistic and social impulses and the exhibition of the inner unity of the two. The contemplative type, beginning in an unsocial withdrawal from the world, culminates in ecstasy, which tends to the dissolution of the individual consciousness, while the active type leads to a fanatical effort after social uniformity and ends by destroying its own goal. The nature of religion is shown in both types, but more clearly in the mystical or contemplative life. Here three stages of the progressive dissolution are noted: First, social relations are lost from consciousness; then intellect ceases to function, and there is left, finally, the characteristic affective state of the ecstatic with its tendency to unconsciousness. From this the inference is drawn that the affective element in religion reveals the true character and the primordial rôle of the religious im-

pulse. The tranquility of the ecstatic, with its extinction of all difference and antithesis, shows religion to be an effort to unify and systematize an unstable consciousness. The view is apparently taken that religious sentiment has its genesis in organic discomfort. I say 'apparently,' because the method of treatment leaves one in doubt as to where, if at all, the author draws the line between what is normal and what is morbid in religious sentiment. In any case, he holds that the essential fact in religion is the effort to secure, through a higher, directing power, unity and system in an otherwise incoherent manifold of consciousness.

This yields him a clue to the function of religion. Inasmuch as 'personality is not an entity,' but 'results from a coördination of states that are incessantly renewed,' the fact of coördination demands an explanation. Murisier sees in religion "the directive idea of the evolution of personality," not the only idea of its kind, but the earliest and, with the great majority of men, the most efficacious one. In a parallel way, religion performs a function of social coördination, so that unity and system is its goal in both directions. These conclusions, which are based upon a rather narrow range of facts, are more suggestive than conclusive. They offer, at least, a timely contrast to Marshall's reduction of religion to a social instinct that works to the actual disadvantage of the individual.

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INHIBITION.

Untersuchungen über psychische Hemmung. G. Heymans. Zeitschrift f. Psychol. u. Physiol. d. Sinnesorgane, Bd. 21, Heft 3, and Bd. 26, Heft 5 u. 6 (1899 and 1901). Pp. 321–359, 305–382.

The investigation of no problem could be more timely than of this one of the general laws of inhibition. The title suggests the point of view from which the results of the experiments are interpreted. Doubtless inhibition is always a psychophysical fact and ultimately must be interpreted in psychophysical terms, but it is useful, if only negatively, to have the subject approached from the purely psychical point of view.

By psychical inhibition the author signifies the general fact of the reduction in intensity or the complete obliteration of one conscious content by another. He regards his experiments in the light only of pioneer work in opening up a new field, but intimates that his law of psychical inhibition applies to a much wider range of facts than he is

able to take up in his own researches, which, stated broadly, are a study of the effect of the greater upon the smaller of two stimuli differing either in quality or in intensity and applied simultaneously to the sensory surface.

The phenomena of inhibition are various in character, ranging from the general facts of the limitation of the field of consciousness and the principle of contrast in the affective life to the specific inhibitive aspect of attention and the measurable modifying effect of one stimulus upon another. The inhibiting effect of a greater upon a smaller pain has long been recognized. The inhibiting effect of one sensation or idea upon another is seen in the ticking of the clock which becomes lost in the sound of the piano, or in the student who, absorbed in his studies, forgets an appointment, or in the soldier who in the heat of the battle does not feel the pain of his wounds. If you shut both eyes you have the sensation of black; if then you open one eye the sensation of light of that eye completely inhibits the sensation of black of the other eye. The author also relates the facts disclosed by his studies to the facts of attention and interest—to the general fact that the interesting process, *i. e.*, the process attended to, becomes clearer and more distinct than the rest of consciousness, with the correlative consequence that the processes attended from become less clear and distinct.

The fundamental question in these investigations was the nature of the laws which control the inhibiting effect of one sensation upon another. The measure of this inhibiting effect was the raising of the stimulus threshold. The specific question thus becomes: According to what law does the raising of the stimulus threshold follow the increase in the intensity of the inhibiting stimulus? Experiments were made on color, taste, sound, pressure, and light sensations. Two methods were followed: the application of stimuli of different qualities to the same sensory surface, and the application of qualitatively identical stimuli to different sensory surfaces. Both methods give the result that the weaker are inhibited by the stronger sensations in proportion to the intensity of the latter.

The author devotes considerable space to the discussion of the question whether these inhibiting phenomena are to be interpreted as physiological or as psychological. He concludes, in accordance with his "idealistic monism" (in which the distinction between the psychical and the physical seems to reduce itself to the distinction between central and peripheral processes in the nervous system), that they must be interpreted as psychological. His classification of these phenomena

as exclusively psychical, and especially his apparent identification of the psychical with the cerebral, are, of course, metaphysical interpretations of the data which his useful researches have brought to light. While interesting, yet these generalizations are the least valuable part of his work. They are rather deductions from his (materialistic?) idealism in philosophy than inductions from his scientific experiments in psychology.

He explains the fact of the lack of correspondence of the absolute stimulus threshold and the sensation threshold by his law of inhibition, *i. e.*, by the simultaneous demand made upon the attention by concurrently acting stimuli. He rejects the hypothesis of neural resistance or inertia as being merely theoretical and unsubstantiated by facts. It is certainly an interesting suggestion that the ultimate explanation of the lag of sensation behind stimulus is due to the reciprocally inhibitive effect of concurrently operative stimuli. That is, the stimulus threshold, so-called, simply marks the limit of the eliminable effects of the simultaneously acting stimuli.

According to Heymans, every idea in the widest sense of the term comes to have a certain inhibitory power and resistance which depends for its intensity on different circumstances of emotional tone, etc., and which can be strengthened by the concentration upon it of the voluntary attention. If the intensity and feeling tone of an idea is small, it will have an inhibitory power only if, and in so far as, the attention is directed upon it, but an idea of greater intensity and tone may not need this concentration of the voluntary attention. The important conclusion which he comes to is that instead of sensations increasing in proportion to the logarithm of the stimuli they increase in direct proportion to the stimuli.

That is, Fechner's law is shown to be true only under certain conditions; in fact, to be true only because of the constant reciprocally inhibitory influence of all the contents of consciousness at any given time. Take away such concurrently acting influences and it is no longer true that the sensations increase in proportion to the logarithm of the stimuli, but in direct proportion to the intensity of the stimuli. To be sure, this also is a purely theoretical law which finds no exact illustration in actual experience, but inasmuch as both laws are theoretical in this sense, this limitation on Fechner's law, if true, is certainly an important contribution to the psychology of sensation.

Fechner's law is shown to be, not a necessary deduction from, but an unwarranted interpretation of Weber's law so-called, which is simply a formulation of certain facts. The author quotes Hering's

objection to the Fechnerian hypothesis, and he puts his own in relation to the researches of Merkel and Ament as casting doubt upon the logarithmic interpretation of Weber's law. He sets aside Wundt's principle of the purely relative measure of intensities, and concludes by applying his own law of inhibition, whereby the difference limen becomes regarded as essentially an example of inhibition, and Weber's law as a special limiting case of the same. According to Weber's law the difference sensation is a proportional part of the stimulus. According to Heyman's law the sensation increases in intensity in direct proportion to the increase in intensity of the stimulus. Furthermore, according to his law, in its application to the difference limen, the inhibited sensation is proportional to the intensity of the inhibiting sensation. Hence, in the case where the difference sensation is inhibited, it is proportional to the inhibiting sensation, and therefore to the stimulus (Weber's law), since, according to Heyman's law, the sensation and stimulus are throughout proportional. Thus Weber's law appears simply as a corollary of this fundamental law of inhibition —that weaker sensations are inhibited by stronger sensations to a degree proportional to the intensity of the latter.

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FLUCTUATIONS OF THE ATTENTION AND RHYTHM.

Untersuchungen über die sogenannten Aufmerksamkeitsschwankungen. E. WIERSMA. Zeit. f. Psych. u. Phy. d. Sinn., Bd. 26, Hft. 3 u. 4. P. 168.

This is the first of a series of articles that Dr. Wiersma proposes to submit on the problem of the fluctuations of the attention. It considers the course of the fluctuations in normal life with stimuli of different intensities. The second will be devoted to a study of the phenomenon in normal subjects under the influence of bodily and mental strain or under the influence of drugs, and the third to pathological cases.

In this investigation Dr. Wiersma worked first to determine the influence of the intensity of the stimulus upon the course of the wave. He found that with an experimental period of five minutes, the period of rotation of his drum, there was constant increase in the time that the stimulus was imperceptible as he decreased the intensity of his stimulus. For sight the extreme stimuli were related as 1 to 3 and the corresponding times of invisibility were 197.9 to 16.1, and there was a close correspondence in the results for the other senses. Dr.

Wiersma, however, overlooked the fact that Marbe had already worked with this phase of the question with the same result, although the investigation had not been so extensive.

Even more interesting are the effects of practice and fatigue during the time of the experiment. This was brought out by dividing each of the experimental series into three parts and comparing the total times of invisibility for the different portions of the curve. The course of the curve varies for the two subjects. Dr. Wiersma shows the effect of practice in decreasing the time of invisibility at first and only later the effect of fatigue, while Professor Heymans showed the effects of fatigue from the beginning. They also plotted the results of different series in a single sitting. The experiments continued for five minutes and were separated by resting periods of eight minutes. In these curves the results are not consistent, but in general Dr. Wiersma shows the effect of practice throughout the period, while Professor Heymans shows practice first, then fatigue. The results also show a difference between day and night workers that is typical.

The experiments add another bit of evidence for the subjective origin of the attention wave and serve to confirm the conclusion that the course of the attention wave may be used as a measure of the efficiency of the attention or of the tonus of the sensory cortical cells. If Dr. Wiersma had used the ratio between the periods of visibility and invisibility as his measure instead of the total time of invisibility it would be possible to compare the results for experimental series of unequal length.

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A Genetic Study of Rhythm. C. R. SQUIRE, Ph.D., late scholar in psychology at Cornell University. Amer. Jour. Psych., XII., 492–589. 1901.

Dr. Squire contributes to the discussion of rhythm in two ways. On the genetic side she presents an order of development among rhythmic forms on the basis of ease in speaking them. On the analytical side she criticises Wundt's definition of rhythm as a form of feeling and herself defines rhythm as a perception with certain characteristics.

The method of investigation was to observe children of the first, fourth and seventh grades as to the manner in which they read the syllable 'me' repeated 30 times. These observations were supplemented by a series of records made by three children reading the same syllable into a Rousselot microphone. Temporal, intensive and pitch

differences were recorded both when the subjects grouped freely and when they were directed to read with a trochaic or other accent.

The author finds a tendency to 'primary' rhythm or ungrouped succession first manifested. When grouping begins she believes the spondee is the simplest form, the group being set off solely by longer intervals. The trochee, instead of being the simplest group, comes second in her classification, and then follow the iambus, dactyl, anapæst and amphibrac in order. It is to be noted that her conclusion that the iambus is simpler than the dactyl is not borne out by the record of errors in repeating the different forms, page 534.

To test the subjective effect of change in pitch a modified form of the Sanford apparatus was used. The subject listened to sounds collected by resonators from *a* and *c* electric forks. The author concludes that pitch is not a determinant of rhythm independent of its interpretation as a difference in intensity.

Breathing curves were recorded when the subjects talked into the microphone, but it is doubtful if they show any functional connection between the perception of rhythm and respiration. The use of the lungs in speaking rhythms probably accounts for the effects found. Observations on concomitant movements and the effect of chorus reading are suggestive but not conclusive.

Dr. Squire argues that rhythm is not a state of feeling because it may occur without feeling, does not become blunted by repetition, is not accompanied by the affective curve and has limits and differences in complexity which feeling fails to explain.

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TOUCH.

Ueber die Flächenempfindung in der Haut. Helen B. Thompson und Katharina Sakijewa. Zeitschrift für Psychologie und Physiologie der Sinnesorgane, December, 1901, XXVII. Pp. 187–199.

This article reports a study of judgments of the size of surfaces touching the skin, and the influence of pressure on these judgments. The experiments have the merit of simplicity, and the observations are interesting and valuable. The apparatus consisted of pieces of cork of different sizes attached to an instrument for measuring the pressure. With these the discriminating power of different parts of the body was investigated. The pressures used were 20, 70, 100, 150, and 250 gm. It was observed that few judgments were purely

judgments of size. The subjects who knew that the pressures of any two surfaces to be compared were always equal were likely to base their judgments on the subjective feeling of difference in pressure. On parts where the tissue beneath the skin is soft it is difficult to tell just what are the chief factors. The smaller surface sinks deeper and this seems to aid in judging. Where the bones are close to the skin the judgment is in the highest degree based on surface sensation. This is probably because the edges are felt more plainly here than on soft tissue. Changes of pressure between 20 and 250 gm. make but little difference so long as the pressures of the two surfaces to be compared remain equal. The discriminating ability of all parts of the skin not trained in touch is about the same.

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GENETIC.

Experimentelle Untersuchungen über die Gedächtnissentwickelung bei Schulkindern. Marx Lobsien. Zeitschr. für Psychologie u. Physiologie d. Sinnesorgane, Band 27, Heft 1. Pp. 34–76.

This investigation shows the manner in which the memory of school children develops from year to year and also the influence of content and sex upon memory. The experiments were made in the Kiel primary school upon 462 children (238 boys, 224 girls) ranging in age from 9–14½ years. The work was suggested by the research of Dr. Netschajeff with the school children of St. Petersburg (*Zeitschrift*, Band 24, Heft 5) and in general followed the same method. A series of nine successive stimuli was given to the children, who were then required to write down as much of the series as they could remember. Eight such series were given to each pupil. Series I. consisted of nine objects—newspaper, key, handkerchief, glass, table, box, book, hand, chalk. Series II. consisted of nine sounds—clapping the hands, knocking, tearing paper, stamping, whistling, ringing a bell, rolling a ball, jingling keys, humming. Series III. was nine spoken numbers—37, 68, 54, 27, 63, 96, 45, 28, 17. Series IV. was nine spoken words connected with visual ideas—Blitzstrahl, Wandkalender, Zifferblatt, Fensterbank, Wandteller, Mondscheibe, Sonnenstrahl, Feuerschein, Himmelsblau. Series V. was nine spoken words connected with auditory ideas—Schutz, Gekreisch, Gebell, Donner, Gebraus, Krachen, Gebrüll, Pfeifen, Geknall. Series VI. was nine spoken words connected with touch images—kalt, weich, rund, glatt, heiss, rauh, spitz, kühl, scharf. Series VII. was nine spoken words connected with the emotions—Sorge, Feigheit, Hoffnung, Zweifel,

Hunger, Angst, Freude, Reue, Neid. Series VIII. was nine spoken words which were meaningless sounds for these children—auditiv, simultan, subjectiv, Transaction, Lyceum, Quantität, Integral, Diffusion, Attraction.

The results of these tests are given in 67 tables and curves. From these the following conclusions are drawn: (*A*) When merely the amount recalled is considered. (1) Memory for each series improves from year to year, but not by constant amounts—Series I., III., V., VI., VII. with both boys and girls show acceleration at 11 and retardation at 12, Series II. shows acceleration for the girls and retardation for the boys at 12, Series IV. shows acceleration for boys and retardation for the girls at 12, Series VIII. shows acceleration for both boys and girls at 12. (2) The girls, with few exceptions, exceed the boys in ability to recall—Series I. and II. show the largest sex difference at 12, the least at 11, Series III., IV., V. and VII. show less sex difference at 12 than at 11, Series VI. and VIII. show little sex difference. (3) Objects and numbers are best recalled by both sexes, words connected with emotions and meaningless words are most difficult to recall. (4) Boys show greatest improvement in Series VII. and least improvement in Series II., girls improved most in Series IV. and least in Series I. (probably because there was so little room for improvement in this series). (*B*) Measured by the exactness with which the order of stimuli in each test is reproduced. (1) There is a general increase in the accuracy of recall, retardation at 10 with subsequent acceleration for both sexes is shown in Series III., IV., V. and VII., retardation at 11 followed by acceleration in both sexes is shown in Series I. and VIII., and in Series II. and VI. the girls show retardation at 10, the boys at 11. (2) Greater accuracy of recall is shown by girls in Series I., II. and V., by the boys in Series VI., VII. and VIII. (3) Objects and numbers are most accurately recalled by both sexes, sounds and meaningless words show least accuracy. (4) Both sexes show greatest improvement in accuracy of recalling objects and numbers, and least improvement in sounds, meaningless words and words connected with the emotions.

The tables and curves contain a number of errors which, however, may be discovered and corrected by careful cross-reference, while a clear resume would add much to the value of the paper.

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COMPARATIVE.

Der Gesang der Vögel, seine anatomischen und biologischen Grundlagen. DR. VALENTIN HAECKER. Jena, Fischer. 1900. Pp. vi + 102.

After presenting a short discussion of the structure of the vocal apparatus of birds and after paying tribute to the contributions of Darwin, Wallace, Spencer, Weismann, Jaeger and Groos on the subject of bird song, the author proceeds to show that the present dimorphism evidenced in the song of male and female is the result of a differentiation, a division of labor, the original condition having been monomorphic. In this process of differentiation a new instinct was evolved, viz., the coquetry, hesitation or passiveness of the female. In the same way it may be said that on the part of the male a new instinct was evolved, viz., the conduct characteristic of courtship. The unusual activity of the male fulfils at least two functions, heightening by a process of auto-stimulation the individual's own sexual excitement and stimulating reflexly certain activities in the female. As in the so-called social gatherings of the paramecia the active movements of the newcomers are inhibited by the carbonic acid excreted by those already present, so the courtship activities of the male act upon the female, *i. e.*, reflexly. Groos had already spoken of *unconscious choice* in this respect; differential reflex action upon the presence of certain stimuli, following the terminology suggested by Lloyd Morgan and the experimentation of Loeb, seems, however, the better term to use. Granted then that the phenomena of courtship in the bird world are instances of reflex action, it follows that sexual selection may be subordinated to natural selection in so far as there would be a survival of those courtship or sexual reactions which possessed the greater survival value for the species.

The utility or survival value of the many specialized, differentiated forms of bird song is discussed with originality. Certain vocal utterances are useful as companionship or flock notes, aiding these most mobile creatures to a closer union and association, their powers of flight giving rise to a dissociating or disintegrating tendency. Warning notes possess survival value in many ways. Many atavistic characteristics, such as the cackling of the hen as explained by Hudson, could be cited. Certain vocal utterances, like human language, are recognition marks of species, of subdivisions of species and of individuals. Attention is called to the differences in meaning in the varying pitch and tone of the same note. Just as orators and singers use those unusual modulations usually associated with emotional life, so in bird life the use

of certain unusual notes induce definite differential emotional reactions. Signal cries are the bugle calls for the migration advance, other notes are pairing notes in the spring. Many notes and melodies which are useful for certain purposes at one time are used at other times and are thus instances of correlation of function. Haecker thinks that these out-of-season notes may be useful as practice. Similar, recurring, seasonal atmospheric conditions may also release reactions which function normally, say, at the pairing time. Song notes are usually intended for other ears than those of the singer. Many other interesting observations are made, such as the growth of the courtship dance out of the struggle and fight of rivals (Kampf und Tanz lassen sich nicht von einander trennen), etc.

The term song instinct, it may be mentioned, should be modified to song activity, since notes, melodies and rhythms are sometimes acquired.

ARTHUR ALLIN.

UNIVERSITY OF COLORADO.

NEW BOOKS.

The Mental State of Hystericals. PIERRE JANET. With a preface by J. M. CHARCOT. Translated by CAROLINE ROLLIN CORSON. New York and London, G. P. Putnam's Sons. 1901. Pp. xviii + 535.

The Basis of Social Relations. DANIEL G. BRINTON. Edited by LIVINGSTON FARRAND. New York and London, G. P. Putnam's Sons. 1902. Pp. xxi + 204. $1.50.

L'Audition. PIERRE BONNIER. Paris, Octave Doin. 1901. Pp. 275.

Principles of Western Civilization. BENJAMIN KIDD. New York and London, The Macmillan Company. 1902. Pp. 538.

Typical Modern Conceptions of God. JOSEPH ALEXANDER LEIGHTON. London and Bombay, Longmans, Green & Co. 1901. Pp. x + 190.

Mental Growth and Control. NATHAN OPPENHEIM. New York and London, The Macmillan Company. 1902. Pp. ix + 296. $1.00.

Lectures and Essays. WILLIAM KINGDON CLIFFORD. Edited by LESLIE STEPHEN and Sir FREDERICK POLLOCK. London and New York, The Macmillan Company. 1901. Vol. I. Pp. 409. Vol. II. Pp. 342.

Intuitive Suggestion. J. W. THOMAS. London and Bombay, Longmans, Green & Company. 1901. Pp. 160.

Russian Political Institutions. MAXIME KOVALEVSKY. University of Chicago Press. 1902. Pp. 299.

The Study of Religion. MORRIS JASTROW, Jr. London, Walter Scott; New York, imported by Charles Scribner's Sons. 1901. Pp. xiv + 451.

The Evolution of Sex. PATRICK GEDDES and J. ARTHUR THOMSON. London, Walter Scott; New York, imported by Charles Scribner's Sons. 1901.

The Criminal. HAVELOCK ELLIS. London, Walter Scott. 1901. Pp. xix + 419.

NOTES.

MR. W. E. JOHNSON, of King's College, Cambridge, has been appointed to the lectureship in moral science established as a memorial to Professor Sidwick.

M. PIERRE JANET has been appointed to the chair of psychology in the Collège de France, vacant by the retirement of Professor Ribot.

PROFESSOR VON KRAFFT-EBING is about to retire from the chair of psychiatry at Vienna and will be succeeded by Professor W. von Jauregg.

WE regret to record the death of Dr. Robert Adamson, since 1895 professor of logic and rhetoric at Glasgow University, and previously professor of logic and mental philosophy at Owen's College, Manchester.

THE American Philosophical Association will hold its first meeting at Columbia University during Easter week.

PROFESSOR HUGO MÜNSTERBERG, as chairman of the philosophical department of Harvard University, is making special efforts to secure funds for the erection of a building for the department, to be known as Emerson Hall. Plans have been drawn by Mr. A. W. Longfellow, according to which the hall is to be a three-story structure, of red brick. On the first floor there will be small recitation rooms and one large lecture hall, seating 400 students. The rest of the floor will be taken up by a philosophical library, comprising an extensive collection of philosophical works. The second story will contain small recitation rooms and seminary rooms for advanced work. The entire third floor will be used for a psychological laboratory. There will be one large room, where work of a general character may be done. The rest of the laboratory will be divided into fifteen research rooms.

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THE PSYCHOLOGICAL REVIEW.

INTERPRETATION OF SAVAGE MIND.

BY PROFESSOR JOHN DEWEY,
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The psychical attitudes and traits of the savage are more than stages through which mind has passed, leaving them behind. They are outgrowths which have entered decisively into further evolution, and as such form an integral part of the framework of present mental organization. Such positive significance is commonly attributed, in theory at least, to animal mind; but the mental structure of the savage, which presumably has an even greater relevancy for genetic psychology, is strangely neglected.

The cause of this neglect I believe lies in the scant results so far secured, because of the abuse of the comparative method—which abuse in turn is due to the lack of a proper method of interpretation. Comparison as currently employed is defective—even perverse—in at least three respects. In the first place, it is used indiscriminately and arbitrarily. Facts are torn loose from their context in social and natural environment and heaped miscellaneously together, because they have impressed the observer as alike in some respect. Upon a single page of Spencer,[1] which I chanced to open in looking for an illustration of this point, appear Kamschadales, Kirghiz, Bedouins, East Africans, Bechuanas, Damaras, Hottentots, Malays, Papuans, Fijians, Andamanese—all cited in reference to establishing a certain common property of primitive minds. What would we think of a biologist who appealed successively to some external charac-

[1] 'Sociology,' I., p. 57.

teristic of say snake, butterfly, elephant, oyster and robin in support of a statement? And yet the peoples mentioned present widely remote cultural resources, varied environments and distinctive institutions. What is the scientific value of a proposition thus arrived at?

In the second place, this haphazard, uncontrollable selection yields only static facts—facts which lack the dynamic quality necessary to a genetic consideration. The following is a summary of Mr. Spencer's characterizations of primitive man, emotional and intellectual:

He is explosive and chaotic in feeling, improvident, childishly mirthful, intolerant of restraint, with but small flow of altruistic feeling,[1] attentive to meaningless detail and incapable of selecting the facts from which conclusions may be drawn, with feeble grasp of thought, incapable of rational surprise, incurious, lacking in ingenuity and constructive imagination.[2] Even the one quality which is stated positively, namely, keenness of perception, is interpreted in a purely negative way, as a character antagonistic to reflective development. "In proportion as the mental energies go out in restless perception, they cannot go out in deliberate thought."[3] And this from a sensationalist in psychology!

Such descriptions as these also bear out my first point. Mr. Spencer himself admits frequent and marked discrepancies (*e. g.*, pp. 56, 59, 62, 65, etc.), and it would not be difficult to bring together a considerable mass of proof-texts to support the exact opposite of each of his assertions. But my point here is that present civilized mind is virtually taken as a standard, and savage mind is measured off on this fixed scale.

It is no wonder that the outcome is negative; that primitive mind is described in terms of 'lack,' 'absence': its traits are incapacities. Qualities defined in such fashion are surely useless in suggesting, to say nothing of determining, progress, and are correspondingly infertile for genetic psychology, which is interested in becoming, growth, development.

[1] *Ibid.*, pp. 59, 60, 63, 69, 71.
[2] *Ibid.*, pp. 79, 82, 85–87.
[3] *Ibid.*, p. 77.

The third remark is that the results thus reached, even passing them as correct, yield only loose aggregates of unrelated traits—not a coherent scheme of mind. We do not escape from an inorganic conglomerate conception of mind by just abusing the 'faculty' psychology. Our standpoint must be more positive. We must recognize that mind has a pattern, a scheme of arrangement in its constituent elements, and that it is the business of a serious comparative psychology to exhibit these patterns, forms or types in detail. By such terms, I do not mean anything metaphysical; I mean to indicate the necessity of a conception such as is a commonplace with the zoölogist. Terms like articulate or vertebrate, carnivor or herbivor, are 'pattern' terms of the sort intended. They imply that an animal is something more than a random composite of isolated parts, made by taking an eye here, an ear there, a set of teeth somewhere else. They signify that the constituent elements are arranged in a certain way; that in being co-adapted to the dominant functions of the organism they are of necessity co-related with one another. Genetic psychology of mind will advance only as it discovers and specifies generic forms or patterns of this sort in psychic morphology.

It is a method for the determination of such types that I wish to suggest in this paper. The biological point of view commits us to the conviction that mind, whatever else it may be, is at least an organ of service for the control of environment in relation to the ends of the life process.

If we search in any social group for the special functions to which mind is thus relative, occupations at once suggest themselves.[1] Occupations determine the fundamental modes of activity, and hence control the formation and use of habits. These habits, in turn, are something more than practical and overt. 'Apperceptive masses' and associational tracts of necessity conform to the dominant activities. The occupations determine the chief modes of satisfaction, the standards of suc-

[1] We might almost say, in the converse direction, that biological genera are 'occupational' classifications. They connote different ways of getting a living with the different instrumentalities (organs) appropriate to them, and the different associative relations set up by them.

cess and failure. Hence they furnish the working classifications and definitions of value; they control the desire processes. Moreover, they decide the sets of objects and relations that are important, and thereby provide the content or material of attention, and the qualities that are interestingly significant. The directions given to mental life thereby extend to emotional and intellectual characteristics. So fundamental and pervasive is the group of occupational activities that it affords the scheme or pattern of the structural organization of mental traits. Occupations integrate special elements into a functioning whole.

Because the hunting life differs from, say, the agricultural, in the sort of satisfactions and ends it furnishes, in the objects to which it requires attention, in the problems it sets for reflection and deliberation, as well as in the psycho-physic coördinations it stimulates and selects, we may well speak, and without metaphor, of the hunting psychosis or mental type. And so of the pastoral, the military, the trading, the manually productive (or manufacturing) occupations and so on. As a specific illustration of the standpoint and method, I shall take the hunting vocation, and that as carried on by the Australian aborigines. I shall try first to describe its chief distinguishing marks; and then to show how the mental pattern developed is carried over into various activities, customs and products, which on their face have nothing to do with the hunting life. If a controlling influence of this sort can be made out—if it can be shown that art, war, marriage, etc., tend to be psychologically assimilated to the pattern developed in the hunting vocation, we shall thereby get an important method for the interpretation of social institutions and cultural resources—a psychological method for sociology.

The Australian lives in an environment upon the whole benign, without intense or violent unfavorable exhibition of natural forces (save in alternations of drought and flood in some portions), not made dangerous by beasts of prey, and with a sufficient supply of food to maintain small groups in a good state of nutrition though not abundant enough to do this without continual change of abode. The tribes had no cultivated plants, no

domesticated animals (save the dingo dog), hence no beasts of burden, and no knowledge or use of metals.[1]

Now as to the psychic pattern formed under such circumstances. How are the sensory-motor coördinations common to all men organized, how stimulated and inhibited into relatively permanent psychic habits, through the activities appropriate to such a situation?

By the nature of the case, food and sex stimuli are the most exigent of all excitants to psycho-physic activity, and the interests connected with them are the most intense and persistent. But with civilized man, all sorts of intermediate terms come in between the stimulus and the overt act, and between the overt act and the final satisfaction. Man no longer defines his end to be the satisfaction of hunger as such. It is so complicated and loaded with all kinds of technical activities, associations, deliberations and social divisions of labor, that conscious attention and interest are in the process and its content. Even in the crudest agriculture, means are developed to the point where they demand attention on their own account, and control the formation and use of habits to such an extent that they are the central interests, while the food process and enjoyment as such is incidental and occasional.

The gathering and saving of seed, preparing the ground, sowing, tending, weeding, care of cattle, making of improvements, continued observation of times and seasons engage thought and direct action. In a word, in all post-hunting situations the end is mentally apprehended and appreciated not as food satisfaction, but as a continuously ordered series of activities and of objective contents pertaining to them. And hence the direct and personal display of energy, personal putting forth of effort, personal acquisition and use of skill are not conceived

[1] All these points are important, for the general hunting psychosis exhibits marked differentiations when developed in relation to ferocious beasts; in relation to a very sparse or very abundant food supply; in relation to violently hostile natural forces; and when hunting is pursued in connection with various degrees of agriculture or domesticated herds or flocks. For economy of space, I have omitted reference to the few portions of Australia where the food supply (generally fish in such circumstances) is sufficiently abundant to permit quasi-permanent abodes, though the psychological variations thus induced are interesting.

or felt as immediate parts of the food process. But the exact contrary is the case in hunting. There are no intermediate appliances, no adjustment of means to remote ends, no postponements of satisfaction, no transfer of interest and attention over to a complex system of acts and objects. Want, effort, skill and satisfaction stand in the closest relations to one another. The ultimate aim and the urgent concern of the moment are identical; memory of the past and hope for the future meet and are lost in the stress of the present problem; tools, implements, weapons are not mechanical and objective means, but are part of the present activity, organic parts of personal skill and effort. The land is not a means to a result but an intimate and fused portion of life—a matter not of objective inspection and analysis, but of affectionate and sympathetic regard. The making of weapons is felt as a part of the exciting use of them. Plants and animals are not 'things,' but are factors in the display of energy and form the contents of most intense satisfactions. The 'animism' of primitive mind is a necessary expression of the immediacy of relation existing between want, overt activity, that which affords satisfaction and the attained satisfaction itself. Only when things are treated simply as *means*, are marked off and held off against remote ends, do they become 'objects.'

Such immediacy of interest, attention and deed is the essential trait of the nomad hunter. He has no cultivated plants, no system of appliances and tending and regulating plants and animals; he does not even anticipate the future by drying meat. When food is abundant, he gorges himself, but does not save. His habitation is a temporary improvised hut. In the interior, he does not even save skins for clothes in the cold of winter, but cooks them with the rest of the carcass. Generally even by the water he has no permanent boats, but makes one of bark when and as he needs it. He has no tools or equipment except those actually in use at the moment of getting or using food—weapons of the chase and war. Even set traps and nets which work for the savage are practically unknown. He catches beast, bird and fish with his own hands when he does not use club or spear; and if he uses nets he is himself personally concerned in their use.

Now such facts as these are usually given a purely negative interpretation. They are used as proofs of the incapacities of the savage. But in fact they are parts of a very positive psychosis, which taken in itself and not merely measured against something else, requires and exhibits highly specialized skill and affords intense satisfactions—psychical and social satisfactions, not merely sensuous indulgences. The savage's repugnance to what we term a higher plane of life is not due to stupidity or dullness or apathy—or to any other merely negative qualities—such traits are a later development and fit the individual only too readily for exploitation as a tool by 'superior races.' His aversion is due to the fact that in the new occupations he does not have so clear or so intense a sphere for the display of intellectual and practical skill, or such opportunity for a dramatic play of emotion. Consciousness, even if superficial, is maintained at a higher intensity.[1]

The hunting life is of necessity one of great emotional interest, and of adequate demand for acquiring and using highly specialized skills of sense, movement, ingenuity, strategy and combat. It is hardly necessary to argue the first point. Game and sport are still words which mean the most intense immediate play of the emotions, running their entire gamut. And these terms still are applied most liberally and most appropriately to hunting. The transferred application of the hunting language to pursuit of truth, plot interest, business adventure and speculation, to all intense and active forms of amusement, to gambling and the 'sporting life,' evidences how deeply imbedded in later consciousness is the hunting pattern or schema.[2]

The interest of the game, the alternate suspense and movement, the strained and alert attention to stimuli always changing, always demanding graceful, prompt, strategic and forceful response; the play of emotions along the scale of want, effort,

[1] For good statements by competent authorities of the Australian's aversion to agriculture, etc., see Hodginkson, 'Australia, from Port Macquarie to Moreton Bay,' p. 243; and Grey, 'Two Expeditions,' etc., II., p. 279.

[2] See Thomas' 'The Gaming Instinct,' *American Journal of Sociology*, Vol. VI., p. 750. I am indebted to Dr. Thomas (through personal conversation as well as from his articles) for not only specific suggestions, but for the point of view here presented to such an extent that this article is virtually a joint contribution.

success or failure—this is the very type, psychically speaking, of the drama. The breathless interest with which we hang upon the movement of play or novel are reflexes of the mental attitudes evolved in the hunting vocation.

The savage loses nothing in enjoyment of the drama because it means life or death to him.[1] The emotional interest in the game itself is moreover immensely reinforced and deepened by its social accompaniments. Skill and success mean applause and admiration; it means the possibility of lavish generosity—the quality that wins all. Rivalry and emulation and vanity all quicken and feed it. It means sexual admiration and conquests—more wives or more elopements. It means, if persistent, the ultimate selection of the individual for all tribal positions of dignity and authority.

But perhaps the most conclusive evidence of the emotional satisfactions involved is the fact that the men reserve the hunting occupation to themselves, and give to the women everything that has to do with the vegetable side of existence (where the passive subject matter does not arouse the dramatic play), and all activity of every sort that involves the more remote adaptation of means to ends—and hence, drudgery.[2]

The same sort of evidence is found in the fact that, with change to agricultural life, other than hunting types of action are (if women do not suffice) handed over to slaves, and the energy and skill acquired go into the game of war. This also explains the apparent contradiction in the psychic retrogression of the mass with some advances in civilization. The gain is found in the freed activities of the few, and in the cumulation of the objective instrumentalities of social life, and in the final development, under the discipline of subjection, of new modes of interest having to do with remoter ends—considerations, however, which are psychologically realized by the mass only at much later periods.

[1] Though some writers even say that the savage's interest in the game of hunting is so great that he hunts for the excitement rather than for food. See Lumholtz, 'Among Cannibals,' p. 161 and p. 191.

[2] This collateral development of a different mental pattern in women is a matter of the greatest significance, in itself, in its relation to subsequent developments and in relation to present mental interests.

As to the high degree of skill, practical and intellectual, stimulated and created by the hunting occupation, the case is equally clear—provided, that is, we bear in mind the types of skill appropriate to the immediate adjustments required, and do not look for qualities irrelevant because useless in such a situation.

No one has ever called a purely hunting race dull, apathetic or stupid. Much has been written regarding the aversion of savages to higher resources of civilization—their refusal to adopt iron tools or weapons, for example, and their sodden absorption in routine habits. None of this applies to the Australian or any other *pure* hunting type. Their attention is mobile and fluid as is their life; they are eager to the point of greed for anything which will fit into their dramatic situations so as to intensify skill and increase emotion. Here again the apparent discrepancies strengthen the case. It is when the native is forced into an alien use of the new resources, instead of adapting them to his own ends, that his workmanship, skill and artistic taste uniformly degenerate.

Competent testimony is unanimous as to the quickness and accuracy of apprehension evinced by the natives in coming in contact even for the first time with complicated constructive devices of civilized man, provided only these appliances have a direct or immediate action-index. One of the commonest remarks of travelers, hardly prepossessed in favor of the savage, is their superiority in keenness, alertness and a sort of intelligent good humor to the average English rustic. The accuracy, quickness and minuteness of perception of eye, ear and smell are no barren accumulation of meaningless sense detail as Spencer would have it; they are the cultivation to the highest point of skill and emotional availability of the instrumentalities and modes of a dramatic life. The same applies to the native's interest in hard and sustained labor, to his patience and perseverance as well as to his gracefulness and dexterity of movement—the latter extending to fingers and toes to an extent which makes even skilled Europeans awkward and clumsy. The usual denial of power of continued hard work, of patience and of endurance to the savage is based once more upon trying him by a foreign standard—interest in ends which involve a long series of means

detached from all problems of purely personal adjustment. Patience and persistence and long-maintained effort the savage does show when they come within the scope of that immediate contest situation with reference to which his mental pattern is formed.

I hardly need say, I suppose, that in saying these things I have no desire to idealize savage intelligence and volition. The savage paid for highly specialized skill in all matters of personal adjustment, by incapacity in all that is impersonal, that is to say, remote, generalized, objectified, abstracted. But my point is that we understand their incapacities only by seeing them as the obverse side of positively organized developments; and, still more, that it is only by viewing them primarily in their positive aspect that we grasp the genetic significance of savage mind for the long and tortuous process of mental development, and secure from its consideration assistance in comprehending the structure of present mind.

I come now to a brief consideration of the second main point—the extent to which this psychic pattern is carried over into all the relations of life, and becomes emotionally an assimilating medium. First, take art. The art of the Australian is not constructive, not architectonic, not graphic, but dramatic and mimetic.[1] Every writer who has direct knowledge of the Australian corroborees, whether occasional and secular, or state and ceremonial, testifies to the remarkable interest shown in dramatic representation. The reproduction by dances, of the movements and behavior of the animals of the chase is startling. Great humor is also shown in adapting and reproducing recent events and personal traits. These performances are attended with high emotional attacks; and all the accompaniments of decoration, song, music, spectators' shouts, etc., are designed to revive the feelings appropriate to the immediate conflict-situations which mean so much to the savage. Novelty is at a distinct premium; old songs are discarded; one of the chief interests at an intertribal friendly meeting is learning new dance-songs;

[1] There are of course pictures, but comparatively speaking, few and crude. Even the carvings, if originally pictorial, have mostly lost that quality, and become conventional.

and acquisition of a new one is often sufficient motive for invitation to a general meeting.

The ceremonial corroborees are of course more than forms of art.[1] We have in them the sole exception to the principle that the activities of the hunter are immediate. Here they are weighted with a highly complicated structure of elaborated traditional rites—elaborated and complicated almost beyond belief.[2] But it is an exception which proves the rule. This apparatus of traditionary agencies has no reference to either practical or intellectual control, it gets nowhere objectively. Its effect is just to reinstate the emotional excitations of the food conflict-situations; and particularly to frame in the young the psychic disposition which will make them thoroughly interested in the necessary performances.[3]

It is a natural transition to religion. Totemism and the abundance of plant and animal myths (especially the latter) and the paucity of cosmic and cosmogonic myth testify to the centering of attention upon the content of the combat, or hunting situation. It would be absurd to attempt in a parenthesis an explanation of totemism, but certainly any explanation is radically defective which does not make much of the implication of tribe and animal in the same emotional situation. Hunter and hunted are the factors of a single tension; the mental situation cannot be defined except in terms of both. If animals get away, it is surely because they try; and if they are caught it is surely because after all they are not totally averse—they are friendly. And they seal their friendliness by sharing in one of the most intense satisfactions of life—savory food to the hungry. They are, as a matter of fact, co-partners in the life of the group. Why then should they not be represented as of close kin? In any case, attention and interest center in animals more persistently than in

[1] It is, of course, a historic fact that the actual origin of dramatic art (through the Greeks) is in mimetic dances of a festival and ceremonial sort.

[2] The best account is of course Spencer and Gillen. Certain ceremonies take weeks.

[3] Not, of course, that all these ceremonies are initiatory in character; on the contrary, many are 'magical,' intended to promote the productivity of their chief food-supplies. But even these were conducted in dramatic fashion, and in such way as to reproduce the emotional disposition involved in the actual occupational life.

anything else; and they afford the content of whatever concentrated intellectual activity goes on. The food taboos, with their supernatural sanctions, certainly create tensions, or reinstate conflict-situations, in the mind; and thus serve to keep alive in consciousness values which otherwise would be more nearly relegated to the mechanically habitual, or become sensuous, not idealized or emotionalized.

I turn now to matters of death and sickness, their cause, and cure, or, if cure is hopeless, their remedy by expiation. Here the assimilation to the psychosis of the hunting activity is obvious. Sickness and death from sickness are uniformly treated as the results of attacks of other persons, who with secret and strange weapons are hunting their victim to his death. And the remedy is to hunt the hunter, to get the aid of that wonderful pursuer and tracker, the medicine man, who by superior ability runs down the guilty party, or with great skill hunts out the deadly missile or poison lodged in the frame of his victim.

If death ensues, then we have the devices for tracking and locating the guilty party. And then comes actual conflict, actual man-hunting. Death can be avenged only by the ordeal of battle—and here we have the explanation of the wars and warlike performances of which so much has been made. It is, however, now generally admitted that the chief object of these war-like meetings is to reinstate the emotion of conflict rather than to kill. They are, so to speak, psychological duels on a large scale—as one observer says, they are 'fights with a maximum of noise, boast, outward show of courage and a minimum of casualties."[1] But the manouvering, throwing and dodging that take place are a positive dramatic exercise in the utilities of their occupational pursuits.

Finally, as to marriage, and the relations between the sexes. What was said concerning the impossibility of an adequate account of totemism applies with greater force to the problem of the system of group relationships which determine marital possibilities. It is clear, however, that the system of injunctions and restrictions serves to develop a scheme of inhibitions and intensified stimuli which makes sex-satisfaction a matter

[1] Horn, 'Expedition,' Vol. IV., p. 36.

of pursuit, conflict, victory and trophy over again. There is neither complete absence of inhibition, which, involving little personal adjustment, does not bring the sexual sensations into the sphere of emotion as such; nor is there a system of voluntary agreement and affection, which is possible only with a highly developed method of intellectual control, and large outlooks upon a long future. There is just the ratio between freedom and restraint that develops the dramatic instinct, and gives courtship and the possession of women all the emotional joys of the hunt—personal display, rivalry, enough exercise of force to stimulate the organism; and the emotion of prowess joined to the physical sensations of indulgence. Here, as elsewhere in the hunting psychosis, novelty is at a premium, for the mind is dependent upon a present or immediate stimulus to get activity going. It requires no deep scientific analysis to inform us that sex-relations are still largely in the dramatized stage; and the play of emotion which accompanies the enacting of the successive stages of the drama gives way to genuine affection and intelligent foresight only slowly through great modifications of the whole educative and economic environment. Recent writers, I think, in their interest on the institutional side of marriage (for we are going through a period of reading back Aryan legal relationships just as we formerly read back Aryan theogonies and mythologies) have overlooked the tremendous importance of the immediate play of psychic factors congruous to hunting as such.[1]

In conclusion, let me point out that the adjustment of habits to ends, through the medium of a problematic, doubtful, precarious situation, is the structural form upon which present intelligence and emotion are built. It remains the ground-pattern. The further problem of genetic psychology is then to show how the purely immediate personal adjustment of habit to direct satisfaction, in the savage, became transformed through the introduction of impersonal, generalized objective instrumentalities and ends; how it ceased to be immediate and became loaded

[1] For a statement doing justice to the psycho-physic factors involved, see Thomas, Der Ursprung der Exogamie, *Zeitschrift für Socialwissenschaft*, Bd. V., I.

and surcharged with a content which forced personal want, initiative, effort and satisfaction further and further apart, putting all kinds of social divisions of labor, intermediate agencies and objective contents between them. This is the problem of the formation of mental patterns appropriate to agricultural, military, professional and technological and trade pursuits, and the reconstruction and overlaying of the original hunting schema.

But by these various agencies we have not so much destroyed or left behind the hunting structural arrangement of mind, as we have set free its constitutive psycho-physic factors so as to make them available and interesting in all kinds of objective and idealized pursuits—the hunt for truth, beauty, virtue, wealth, social well-being, and even of heaven and of God.

THE ATOMIC SELF.

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Science has long been at work building up the conception of the material world as a mechanical system of things. Many hands have labored to rear the edifice, many still labor, and yet the pile has scarcely risen above its foundations. Only the eye of faith can see its towers and pinnacles rising in stately magnificence and dwell with pleasure upon the unity and harmony of the colossal structure. Those who are most deeply imbued with the spirit of science and who enjoy that breadth of vision denied to the myopic eye of the mere specialist, are apt to exercise this faith and to see the world as a perfect mechanism, while frankly admitting that it is quite beyond the power of science to prove it to be such. On the other hand, there are those, and among them men of great scientific eminence, who do not believe that this faith rests upon a sure foundation. The world of matter, they maintain, will not be proved to be a perfect mechanism, because it is not such.

But, whatever view we may take of the world of matter, however independent we may conceive it to be, we are nevertheless forced to recognize the existence of a realm of minds. The man who insists that nothing exists save matter is foolish—about as foolish as the man who insists that nothing exists save mind. The plain man stands between the two and finds himself in a composite world in which things material and things mental play their proper rôle without crowding each other out of existence. That the chair upon which he sits, the table at which he writes, the pen which he holds, are material things, it seems to him trivial to doubt. That there is such a thing as mechanism he can prove by pulling out his watch. On the other hand, it is *he* that pulled out the watch, a thing that can feel, think, remember, will—in short, a mind. And he cannot conceive any

man in his senses to come seriously to the conclusion that he alone possesses a mind.

That he is right in maintaining the distinction between matter and mind and in holding to the existence of both, careful analysis will only succeed in making more certain. The philosopher who denies his position may see some truth that he does not see, but his denial rests upon an imperfect apprehension of the truth. We have on our hands a world of matter and a realm of minds; neither can be declared non-existent; the only question is, what shall we do with the two? I have said that, to the plain man, the difficulty does not appear to be a serious one, because he builds the two into one system, or, at least, into something resembling a system. He treats minds very much as if they were material atoms and could influence the latter as these influence each other. But to the man who has come to look upon the material world as a perfect mechanism the problem is a far more serious one, for such a conception of the interaction of mind and matter as the above seems to make havoc of the notion of mechanism.

So serious is the difficulty that some of those whose acuteness and whose learning are undisputed have come back from a study of what many philosophers have had to say touching the problem, with a disposition to rest content with the position of the plain man as being, on the whole, the most satisfactory. The plain man appears to give a plain answer to the question, and one not out of harmony with our common experience of things. Why not accept it and let it go at that? The position seems by no means an unreasonable one, at first sight, at least. If, in taking it, one is compelled to deny the assertion of certain persons that the material world is a perfect mechanism, it is easy to point out that these persons can give no adequate proof of their assertion and to hold that they may very well be in the wrong.

But it is evidently unwise to adopt a position without making a careful examination into all that that implies. It is quite possible that such an examination will reveal that one has passed from bad to worse in abandoning philosophy for common sense. Of course, one cannot expect the plain man to realize clearly all that his doctrine implies. We must, hence, try to make

clear to ourselves what he does believe, and then judge whether such beliefs with their implications are what we should elect to adopt as a satisfactory solution of the problem of the relation between matter and mind.

Here I should premise, in the first place, that I lay myself open to easy criticism in trying to make clear what is in its nature vague and fluctuating. A man may hold a thing in mind so dimly and vaguely that he may fail to recognize any clear thought whatever as the thing he had in mind, and may resent having it attributed to him as his own. Moreover, the plain man is not one, but many, and although he may, for certain purposes, be taken generically, he presents specific differences which are not without their significance. I should premise, in the second place, that by the plain man I do not mean the *very* plain man, but the man who has some opinions, at least, on the subject of mind and matter. It must be admitted that he has not gathered his opinions independently from his own experience. Such opinions never are gathered independently. They exude from old philosophies; they are absorbed into theological and ethical systems; they leave their traces upon language and literature; they are taken up again, worked over, and incorporated into text-books for the instruction of young men; they become a part of the common thought of the race, and in the mind of every man of a moderate degree of culture they find a lodgment as part of that heritage from the past which he has accepted as he has accepted his social prejudices and his elementary notions of rights and duties. It may seem to a man that he has direct evidence in his own experience that such opinions are true. He should remember that it also seems to him that he has direct evidence that he does his thinking with his head, and not with some other part of his body. Yet it took the race a long time to discover the true significance of the brain in the animal economy, and many generations of men lived and died without being impressed with this direct evidence at all.

We may, hence, regard the opinions of the plain man on such subjects as the echoes of past philosophies; echoes which he takes for the voice of truth, and which seem to him to be just interpretations of what is given in his experience. If we

go back to these philosophies we shall often find labored attempts to make reasonably clear what he is content to leave wholly vague. It may, consequently, be objected that any attempt to state clearly the opinions of the plain man on the subject of mind and matter and their relation to each other, must result in setting these opinions aside and treating, instead, of those philosophical doctrines in which they have had their origin. The objection is not without force, and yet it is difficult to see how one can make clear what a man believes dimly and vaguely in any other way than by setting forth what his words would mean to him did he see things under a light less dim and uncertain, or what they have meant to others more given to the habit of reflection. It is not worth while to discuss a man's opinions, if it is understood from the outset that the discussion must leave the whole subject as vague as it was before. If any plain man feels aggrieved at my attributing to him doctrines which he is not conscious of holding, I beg him to assume that my words have reference to another and not to him individually. An experience, extending over a considerable number of years, with successive classes of college students representing, on the whole, the more cultivated classes in the community, has confirmed me in the opinion that there are certain philosophical tenets touching the nature of the mind held with a good deal of unanimity even by those who have done no reading in the works of the philosophers, and have no idea of the original sources of the doctrines to which they hold. They are comprehended vaguely; those who maintain them are often thrown into confusion by the first objection urged against any or all of them; but they are nevertheless held to with a good deal of tenacity. These tenets I take the liberty of calling a part of the philosophy of the plain man. Under this heading I include the following beliefs:

1. That the mind is in some sense in the body.
2. That it acts and reacts with matter.
3. That it is a substance with attributes.
4. That it is non-extended and immaterial.

In these statements there is nothing that strikes the average man as absurd or incredible. Taken together they describe

what may fairly be called *the atomic self*, that is, the self or mind vaguely conceived after the analogy of a material atom. It is true that the thing is expressly affirmed to be immaterial, but that only means that the analogy is recognized to be somewhat imperfect.

But one's satisfaction with such statements as these can only endure so long as one does not subject them to careful scrutiny and ask after their precise meaning. In what sense can the mind be regarded as in the body? and what is intended by the statement that it acts and reacts with matter? Let us ask the plain man to look at the boy chasing the dog, whom I have discussed in an earlier paper,[1] with the sharpness of vision there supposed possible. What does he see? He sees an enormously complicated system of material atoms changing their space-relations to each other unceasingly, and in such changes obeying mechanical laws. The whole system of atoms constitutes what we call the boy's body. Each atom is plainly and unequivocally *in* the body, for it is clearly a member of the group, and stands in such and such space relations to the other members. The word *in* has no doubtful meaning when one is speaking of material things. My papers are *in* my desk, that is, they occupy certain definite portions of space, and the wood which composes the desk occupies certain other portions on this side and on that. My body is *in* this room, that is, it occupies a position between the walls, can by moving in this direction touch one of them, and by moving in that, touch another. An analysis of the conceptions of matter and of space reveals[2] that when we speak of a thing as being here or there we are simply assigning to a given group of tactual sensations its position in the vast system of tactual and movement sensations which constitutes the real world in space and time. If I choose to locate a mathematical point in this room, I treat the point as I would treat an atom, and I believe that a line might be drawn from one wall through the point in question to another wall. It seems, then, that to be anywhere, in an intelligible sense of the word, a thing must be material. It must form a part of the material system of things, and this it cannot do without being itself material.

[1] THE PSYCHOLOGICAL REVIEW, March, 1901.

[2] See my paper in the *Philosophical Review*, November, 1891.

Now does anyone suppose that any degree of acuteness in vision would reveal the mind to be in the boy's body as an atom of matter is in it? Such a supposition seems to be quite excluded by the statement that the mind is immaterial. In what sense, then, can the mind be in the body? A careful examination of the plain man's opinion upon this subject reveals the fact that he really does assign to the mind, dimly and vaguely, an atomic 'in'-ness, while refusing to accept all that this implies—perhaps, even, while holding to what flatly contradicts this. The doctrine that the mind is in the body is venerable with age. At first it was a mind that was very unequivocally *in;* it was composed of fine round atoms, highly movable atoms, etc., etc. It could be inhaled and exhaled, and might escape through a gaping wound, as wine spouts through the rent wine-skin. It was a kind of matter and nothing more, having the same right to occupy space that has any other form of matter. Afterwards, it was for centuries still *in* the body, but in a much more indefinite and inconsistent fashion. It was wholly in the whole body, and wholly in every part. This scholastic doctrine I have criticised earlier,[1] and it is not necessary for me to dilate upon it here, further than to say that, to have this collocation of words mean anything to him, a man must think vaguely of "in"-ness, in the proper sense of the word, and must keep what he has in mind very vague. He must think of an immaterial atom, which by virtue of its being an atom can be somewhere, and by virtue of its immateriality can be nowhere in particular, but rather everywhere in general. It is an echo of this doctrine that comes before us as the opinion of the plain man, although he has never heard the words *tota in toto*, and may be shocked by their meaning as explained to him. He thinks of the mind as in the body, much as a material atom is in the body, and yet he does not think that it would be open to direct inspection however acute one's power of vision. He hesitates to localize it very definitely, and would be unwilling to speak of it as exactly at the middle of the straight line joining this atom and that. He shakes his head over the suggestion that, if the mind really is in the body, a line might conceivably be drawn

[1] THE PSYCHOLOGICAL REVIEW, January, 1897.

through two different brains in such a way as to pass through two different minds, whose distance apart might, thus, be accurately determined.

But it may be urged that, however indefinite the plain man's ideas may be, it is scarcely fair to foist upon him the scholastic doctrine of the ubiquity of the mind in the body. The objection is perhaps just, for that doctrine is not completely represented in the echoes of it which came back to us from most men's minds. Yet it should not be forgotten that the more completely one eliminates from one's thought the notion of this absurd ubiquity, and the more earnestly one strives to make the presence of the mind in the body a comprehensible thing, the more plain does it become that what one has in mind is an atomic self, a minute material self, which is present in the body as any material atom is present in a group of such atoms. We can see this well illustrated in the case of Descartes, whose acquaintance with the mechanism of the body led him to attempt an emendation of the scholastic doctrine. He did not deny the ubiquity of the mind, for he was willing to assert, in accordance with the orthodox tradition, that it was united to all the parts of the body 'conjointement.' Nevertheless, he assigned to the mind a 'siège principale' in the little pineal gland in the middle of the brain. Listen to what he has to say touching its behavior in this its inner sanctum:

"Let us here, then, conceive of the soul as having her chief seat in the little gland which is in the middle of the brain, whence she radiates to all the rest of the body by means of the spirits, the nerves, and even the blood, which, participating in the impressions of the spirits, can carry them through the arteries to all the members. And let us remember what has been said above of the mechanism of the body, to wit, that the little threads of our nerves are so distributed to all its parts, that, on occasion of divers movements excited in those parts by the objects of sense, they open in divers ways the pores of the brain, which brings it about that the animal spirits contained in these cavities enter in different ways into the muscles, by means of which they can move the members in all the different ways in which they are capable of being moved; and also that all

other causes, that can move the spirits diversely, can conduct them to divers muscles. Let us add, too, that the little gland which is the chief seat of the soul is so suspended between the cavities that contain these spirits, that it can be moved by them in as many different ways as there are different sensible qualities in the objects; yet that it can also be moved in different ways by the soul, whose nature is such that it receives as many different impressions, *i. e.*, has as many different perceptions as there are different movements in this gland. The mechanism of the body is so constructed that, simply from the fact that this gland is moved in divers ways by the soul, or by whatever cause may be, it pushes the spirits which surround it toward the pores of the brain, which conduct them by the nerves to the muscles, and thus makes them move the members."[1]

"Thus, when the soul wills to call anything to remembrance, this volition brings it about that the gland, inclining itself successively in different directions, pushes the spirits towards divers parts of the brain, until they find the part which has the traces that the object which one wishes to recollect has left there. For these traces are nothing except that the pores of the brain, through which the spirits have formerly taken their course because of the presence of the object, have acquired thereby a greater facility than the others of being opened again in the same way by the spirits which return to them. Thus these spirits meeting these pores enter more easily into them than into the others, by which means they excite a peculiar movement in the gland, which represents to the soul the same object and makes it conscious that it is the one it wishes to recollect."[2]

Can anything be more clearly material than this little mind that sits in the pineal gland? It has its definite place among other material things; it appears to be able to push and be pushed like the veriest bit of matter. Its presence in the body does not seem at all incomprehensible, for it does not appear to be in any wise different from the presence of a pen between a man's fingers, or the presence of a human body in a room. If

[1] 'Les Passions de l'Ame,' Art. 34. The 'spirits' here referred to are, of course, the 'animal spirits,' and nothing immaterial.

[2] Ibid., Art. 42.

one goes on to say that the mind is wholly without extension, is immaterial, and the like, one's thought becomes once more somewhat confused, for one is affirming material presence and in the same breath denying that the thing present is really material. But if one's thought is sufficiently vague, the contradiction is not unpleasantly apparent, and may conveniently be overlooked. The scholastic doctrine tries to make too clear what is meant by *immaterial* presence; it stirs up the contradiction and makes it growl, striking fear to the heart of the beholder. Descartes, in his doctrine of the soul's seat, emphasized the *presence*, and passed over the difficulty about its being immaterial. It goes without saying that if one emphasizes *both* sides of the inconsistent doctrine, and makes both clear, the result cannot but be disconcerting—except to the chosen few who have embraced a philosophy of contradictions, and rejoice in the absurdity of the conclusions to which their reasonings conduct them.

That the attempt to make at all clear the nature of the presence of the mind in the body reveals that what is really at the heart of the plain man's thought is a material presence, may be equally well illustrated by taking a modern instance. No one kept closer to the philosophy of the plain man than the late Dr. McCosh. His works have appealed to a very large number of cultivated persons, not specialists in philosophy, as embodying the most sensible opinions, and the most reasonably conservative, on many subjects with which the philosopher deals. He has never been accused of being a materialist, and he certainly never meant to lend his countenance to those who incline to this type of thought. Yet when he comes to speak of mind and body, and makes the effort to be a little explicit, he is capable of writing as follows:

"The mind is so constituted as to attain a knowledge of body or of material objects. It may be difficult to ascertain the exact point or surface at which the mind and body come together and influence each other, in particular, how far into the body (Descartes without proof thought it to be in the pineal gland), but it is certain that when they do meet mind knows body as having its essential properties of extension and resisting energy." [1]

[1] 'First and Fundamental Truths,' N. Y., 1889, Part II., Book I., Chap. II.

Here we find the scholastic ubiquity stripped away. The mind is not in the body 'in general,' but is located at some unknown distance within the skin. It can *meet* matter; it can *come together* with it, possibly at a point, possibly at a surface. Must it not be a material mind that can act thus? In contemplating the boy's brain as a swarm of atoms, we can at least conceive any two of them as meeting each other. They can lie side by side in space, with no room between them. They can *touch* each other. Whether atoms do actually ever touch each other is not a question with which we need concern ourselves here. We can conceive that they do, and we can use the expressions 'come together' and 'meet' in a perfectly intelligible sense. But suppose one of the atoms to be immaterial, that is, suppose it not to be an atom, a thing that can be touched. What can we mean by a *meeting* between a thing that can be touched and a thing that can not? They can certainly not touch each other, and if not that, what do they do? It is perfectly evident that, in so far as Dr. McCosh's conception seems to the reader satisfactory, it is because he has emphasized the presence of the mind in the usual sense of the word presence, and has passed over the difficulties which arise out of the attempt to combine with this the notion of immateriality.

And if, when one emphasizes the notion of immateriality, that of the presence of the mind fades out into utter indefiniteness, what becomes of the conception of interaction? We can conceive of a new atom being brought into the group of atoms which constitute a human body, and of its interacting with them. This means that it and the others approach to or recede from each other in ways that can be explained by a reference to mechanical laws. Interaction in this sense seems out of the question where one is no longer dealing with material things. But in what sense, then, can we speak of the interaction of mind and body? It is easy to say that when the mind wills, such and such changes take place in the material world; but to say this is simply to go back to the common experience that there is such a thing as volition, and that this is in some way related to the changes that take place in the world of material things. This experience no one cares to deny. It is admitted as frankly by

those who regard the world of matter as a perfect mechanism, as it is by the interactionist. From this experience to the doctrine of the atomic self in the pineal gland or elsewhere is a very long step, and one never made by the plain man independently. When he makes it, he has passed from experience to philosophical theory, and it is perfectly just that this philosophical theory should be expected to stand or fall according as it succeeds in explaining or fails to explain the experience which it undertakes to make comprehensible. It is, then, right that we should ask how this atomic self is to be conceived as setting in motion material atoms. What is its volition? Shall we think of it as a motion? If we do, we are back again within the realm of matter. Shall we deny it to be a motion, and hold that it is a peculiar and indescribable occurrence which takes place *within* the self, and wholly within the self? Then how shall we conceive this change within an immaterial atom to bring about motions in material atoms? The immaterial atom is not spatially present, in any intelligible sense of those words; the change which has taken place is wholly within it; and yet it is to be regarded as the cause of motions in matter. If this does not strike the plain man as a serious difficulty, it is because he sees so dimly that he is unable to recognize a difficulty when he meets one.

But to those who have given the subject careful thought, the difficulty of patching up a mechanism with immaterial cogs and couplings has seemed an enormous one. Descartes appeared to have made reasonably comprehensible the interaction of mind and body when he placed the former in the pineal gland, where it could, so to speak, hold in its hand all the strings of the machine. On the other hand, Descartes had declared the mind to be non-extended, and had made its essence to consist in thought. How could such an entity be conceived to possess a hand material enough to hold material strings at all? This problem had to be faced by Descartes' successors, and, the notion of immateriality winning the day over that of material presence, they felt compelled to deny that it could hold the strings. The mind wills, said one, but it cannot, thereby, directly affect matter; on occasion of its volition, God brings

about changes in material things. The mind perceives things, said another, but not by virtue of their directly affecting it; it sees things in God. The difficulty is as great now as it ever was, and if the plain man is not driven to such extremes by the inconsistency of his doctrine, it is, as I have said, because he does not greatly emphasize the notion of immateriality. His explanation of the interaction of mind and matter can only seem to him an explanation in so far as his thinking is materialistic. No man would attempt to fill in a gap in a series of colors by the insertion of a smell clearly recognized to be such. But a man might talk of completing his color-series in this abnormal way, if he dimly conceived of a smell as being some kind of a color.

It is, hence, sufficiently clear that it is easy to conceive this immaterial atom as present in and interactive with the body, only so long as one dimly thinks of it as material. When one is careful to eliminate from one's thought every suggestion of the material, all positive content seems to vanish. Nor is there a difficulty only with the conceptions of presence and interaction. If it is true that it is hard to conceive of the atomic self as having a rôle to play in the management of the bodily mechanism, it is no less true that it is hard to frame any idea, which shall have even an approach to clearness, of the nature of this immaterial entity and its relation to its ideas. We are told that it is an immaterial substance and that it possesses attributes. But what, in general, is a substance, and what is its relation to its attributes? If we search curiously into this obscure notion we are carried back many centuries in the history of philosophy, and we realize that the opinions of the plain man have their roots in a remote antiquity. We see that it has seemed to many generations of thinking men too evident to require proof, that each thing must consist of a substance with its qualities or attributes. The qualities are color, form, hardness, taste, smell and the like, in the case of certain things, and thinking, remembering, willing and the like, in the case of others. The substance is of a more retiring nature, and does not present itself to direct inspection. Nevertheless, it is there, and it is indispensible. It is *substance*, *substratum*, that which underlies the qualities, that which *has* them. It exists in itself—*per se subsistit*—and they

exist in it as dependent existences. If one will imagine a pin-cushion stripped of those qualities by which we commonly recognize it to be a pin-cushion, its extension, its hardness, its weight, its color, etc., etc., and if we will permit it to retain only the property of holding the pins which are stuck (? !) into it, we shall have something that at least suggests the substance which busied philosophers all through the middle ages, and busies a number of them even at the present time. It has survived some very serious shocks in its day. When Descartes made a feint of sweeping aside all the philosophical prejudices which had come down to him from the past, he was unable to rid himself of this notion. He made the essence of matter to consist in extension, and the essence of mind to consist in thought, but these essences are not in themselves complete and independent. They drag with them as their shadow the substance or substratum which the 'natural light' (a euphemism for inveterate prejudice) convinced Descartes must accompany every quality or attribute.[1] The substances thus brought in play no part in the Cartesian philosophy; throughout the whole four acts they remain behind the scenes. Still they are assumed to be present, and to be in some obscure way indispensible to the drama.

One of the most serious attacks ever made upon this ghostly pin-cushion was made by one of its friends. When John Locke undertook to make clear the distinction between ideas, qualities of things, and substance, he did the last of these a great disservice. He made it too clear that, when one has carefully distinguished between qualities and substance and has set all qualities of whatever sort on the one side and naked substance on the other, the nakedness of the thing is so complete as to resemble the emptiness of a vacuum. One is tempted to ask whether one has anything left at all. We have no idea what substance is, said Locke; we have only an indefinite notion of what it does. It is a 'we know not what,' and its function is to hold together the bundle of qualities which constitute the things we do know. The idea could not have been gained from any experience whatever, and its existence cannot be

[1] *Prin. Philos.*, I. 11.

logically defended.[1] Surely an entity at such a pass has no excuse for existing; we do not know what it is; we have not the faintest idea how it can do what it is supposed to do; the fact of its existence has been assumed without apparent justification. It appears to be made out of whole cloth, if so mere a nothing can be said to be made out of cloth at all, and did it possess a particle of self-respect it would expire and be done with. Curiously enough, it does not expire even in the pages of Locke, which contain poison enough to make way with a dozen such; and it is not surprising that it lurks in the obscurer corners of the mind of the plain man, who may quite fail to see that it is living on through sheer effrontery and in spite of the fact that it has logically died and been buried.

The interesting question is, why does it live on? Why does it seem worth while for men to insist upon the existence of so mere a nonentity? This question we can answer by pointing out that this nonentity is a vampire which draws from the qualities, with the sum total of which it is supposed to be contrasted, the few drops of blood which nourish its equivocal being. John Stuart Mill, in his remarkable chapter on "The Psychological Theory of the Primary Qualities of Matter," has insisted that, when we speak of material substance, we really have in mind the touch-qualities of a thing, qualities which, taken together, form, as it were, an inner nucleus, to which we refer all the other qualities.[2] His analysis is quite in the line of modern psychological investigations, which recognize that the real world in space and time is a world revealed in terms of touch-movement sensations. But Mill might profitably have brought out more clearly the fact that, when we distinguish between a *thing* and its qualities, the *thing* is not clearly recognized by us to be composed of qualities of any sort. It is indefinitely thought of as the possibility of all the qualities, the center from which they emanate, the bond of union between them. It is the group as a group, contrasted with the individuals which compose it. Manifestly, if we carefully put all the individuals aside, the group disappears and we are left without a residue.

[1] 'Essay,' Book I., Chap. IV., § 18; Book II., Chap. XXIII., § 4.
[2] 'An Examination of Sir William Hamilton's Philosophy,' Chap. XIII.

This is what Locke did and he left himself empty-handed. But in so far as Locke still believed in substance, and indefinitely thought of it as a real existence, he did what is done by the plain man, he made an imperfect abstraction, leaving enough of the qualitative to prevent his substance from becoming a mere nothing. He was, of course, inconsistent, but inconsistency comes to be regarded as almost a prerogative of the philosopher by those who read much in the history of philosophy. Material substance remained to Locke enough of a touch-thing to be in this place or that, to be moved about. He thought of it vaguely as one thinks of things that can be touched, and there certainly was dimly present to his mind the core of tactual qualities upon which Mill dwells and which he himself in his moments of clearer thought set over against substance as something to be contrasted with it.

With the useful distinction between substance and qualities I have no quarrel. I wish merely to point out that it is very easy to misconceive the significance of the distinction, and to suppose that the substance is a something that can be set over against the qualities in their totality. It is a little as though one distinguished between the river and all the water that ever flows in the river. And when one falls into the error of treating substance in this way, it is clear that one gains an indefinite meaning for what would otherwise be an empty word, by borrowing something from the bundle of qualities with which the substance is contrasted. When the plain man distinguishes between this table and the qualities of the table, his words undoubtedly mean something to him. The table as substance is not to be accepted as a mysterious and unanalyzable datum in his experience. It is perfectly possible to analyze the conception, and to show what elements are present in his thought. There is present in a vague and shadowy way that core of touch qualities emphasized by Mill, and this is present even when he insists that he is not thinking of qualities at all. Were it not present he would not treat substance as he does, giving it a local habitation, and thinking of it as *in* things.

That this is in his thought when he talks of material substance, and that this content accounts for the satisfaction with

which he comes back to a conception which would otherwise be to him a meaningless abstraction, is sufficiently clear. But what has been said above about the general tendency to give the atomic self a materialistic presence in the body makes it also evident that this is present in his thought even when he is talking about a substance which he assumes to be immaterial. Surely this is illegitimate in the highest degree. An immaterial self must not be represented in our minds by any group of touch-qualities, however indefinite. How, then, shall we think it?

The problem is a very serious one, indeed. How important a part the touch-movement sensations play in a man's notion of material substance, he can make clear to himself by trying consistently to carry out the Lockian abstraction. Here is this table: it is colored, hard, extended. One may think of these qualities as inhering in a substance. Now abstract in thought the color. The table seems to remain; it is a table in the dark. But abstract every degree of hardness, and all extension. The table seems to disappear completely. Yet the hardness and the extension are assumed to be qualities, and distinct from the substance which underlies them. Nevertheless, in their absence, the substance evaporates. Is the substance in itself extended? or is extension only one of its qualities? If it is not in itself extended, how can it 'hold together' this whole expanse of table-top? How can it be, in any intelligible sense of the word, a *substratum?* One can not spread a non-extended entity under an expanse of anything, and if it is not necessary for the substance to be spread under the qualities in any sense at all, why may not the substance of that door support the qualities of this table as well as the qualities of that door? One who travels this road may easily reach the point of maintaining that there is only one substance, and this is next door to maintaining that there is no substance at all, at least in any sense of the word at all analogous to that in which it has been used in the preceding discussion.

Now when a man talks of an immaterial substance he almost forces himself to a Lockian thoroughness of abstraction in his treatment of substance. The dim core of touch-qualites which

has inconsistently remained in his thought and has prevented him from groping in mere emptiness is threatened with total extinction. How is he to think even dimly of this immaterial substance? He feels impelled to assert, in accordance with the ancient tradition, that it is simple and non-extended. But these negative determinations are just the knife that should cut him off from the vague materialistic content that gives its meaning to his conception of substance. His only recourse is to retain at all hazards a little meaning, and allow his thought to grow still dimmer than it was before. If his material substance was the shade of a group of material qualities, his mental substance, the atomic self, is the shade of a shade. So dim is it and so unreal, that he has not the least expectation of attaining to any clear ideas regarding it, and he may even resent the attempt to set it in a sharper light. His notions of it and its ideas and activities are a mere mess of inconsistencies and incomprehensibilities, and with this mess he is content because he does not believe that consistency and clearness can justly be looked for in this corner of the realm of human knowledge. When, therefore, one talks of abandoning the speculations of the philosophers and of coming back to the more sober conceptions of the plain man, it is right that we should ask him to open his eyes and see to what he is coming back. He is not coming back to experience, *i. e.*, to uninterpreted experience. He is abandoning certain speculations for certain others, which, by no means satisfactory in themselves, yet seem satisfactory to a large number of persons, because they are matter of tradition and have come to fit their habits of thought as an old shoe fits the foot.

That there is nothing even moderately clear in this doctrine is written all over its face. We have seen that when we ask what the atomic immaterialistic self is and how we are to conceive it, no answer is forthcoming. It appears to be a shadow of a materialistic shadow. When we ask how it can be present in the body, it becomes evident that, in so far as it is thought of as present, it is thought of as material. Manifestly we must not think of it as material. When we ask how it interacts with matter, no one even pretends to give us information. If, now, we turn in desperation to enquire at least how we are to con-

ceive its relation to its own ideas, we fare no better. What do we mean when we say that it *has* ideas? May we regard the ideas as minute pictures that exist in or on the surface of this substance? A good many intelligent persons can be brought to confess, by means of a little questioning, that they are apt to represent the thing to themselves in this way. But a moment's reflection makes it apparent that this will not serve even to give a hint of the relation which must be conceived to obtain between the atomic self and its ideas. That which is perfectly simple and non-extended cannot have an inside and an outside, and it is not conceivable that anything should be either in it or on it, in any intelligible sense of those words. Moreover, the ideas themselves do not appear to be simple. If I close my eyes and call up in imagination a barber's pole, it seems to stand before me as an extended thing in which white lies beside red and red beside white. Does it mean anything whatever to talk of this composite something as either *in* or *on* a non-extended and simple substance? To be sure, I may maintain that the imaginary barber's pole only *seems* to be extended, and is not really extended at all; but if I do this I fall headlong into a difficulty quite as grave as the one I am seeking to avoid. How can that which is quite simple and non-extended seem to have part out of part? Has it really no parts at all? Am I fed with pure illusion, and is the white not really different and distinct from the red and the red from the white? One may diminish the size of a thing and yet retain certain characteristics which make it possible to distinguish it as a thing of a given class. A small picture of a horse and a large one may both be recognized to be pictures of a horse. But if we annihilate altogether the extension of the picture of a horse, if we conceive it to shrink into the nothingness of a mathematical point, this simple and non-extended something has ceased to be a picture of a horse at all. It is inconceivable that it should represent any creature in the heavens above, in the earth beneath, or in the waters under the earth. When, therefore, the plain man loosely talks of ideas as *small* pictures, he may be speaking unwisely, but he is not talking mere nonsense. It is reserved for him to do this when, laboring under the delusion that it is his duty to put these ideas

in or on a non-extended self, he affirms of them *absolute simplicity* in the hope that this may render his task a less desperate one. We must admit, in his justification, that it does seem somewhat plausible to maintain that it is more difficult to conceive of an extended thing as existing in or on a non-extended thing than to conceive of a non-extended thing as doing this. Still, men have more than one idea at a time, and he who has reduced his ideas to punctual insignificance as a preliminary to incarcerating them in their spaceless cell, must still ask himself how two or more ideas thus bottled can be conceived to remain distinct and distinguishable.

When brought to bay by questions, the plain man may not unreasonably maintain that, in speaking of the relation of the self to its ideas, he uses the words *in* and *on* in a loose sense, and does not intend them to be taken with offensive literalness. We all say in common life that ideas are in the mind, and we do not stop to make clear to ourselves what our words mean. But philosophic theory—and the doctrine of the atomic self is a philosophic theory—has no right to be content with the indefiniteness of thought which may serve a useful purpose in common life. When Berkeley has set forth his doctrine that the things of sense are only ideas, and are, hence, in the mind, he comes face to face with the objection that, if they are extended and yet are in the mind, the mind must be extended. This consequence he is not ready to admit, and he argues that the mind is not extended, for these things are in the mind only 'by way of idea.'[1] What can this mean? Nothing definite. He has fled to the refuge of the plain man—obscurity. Ideas are *in* the mind somehow, but just how cannot be made plain. In the foregoing pages I have tried to make it clear that, when the indefinite thought of the plain man is carefully examined, it is found to be the echo of an ancient materialism or semi-materialism. This gives it its positive content. With this it attempts to combine the statement that the self is immaterial. When great emphasis is laid upon this latter, the positive content of the atomic doctrine is wiped out of existence. But in most men's minds great emphasis is not laid upon this negative

[1] 'Principles of Human Knowledge,' § 49.

element, and they can find satisfaction in the indefinite materialistic notions which they continue to hold touching the substance of the self, its relation to its ideas, its presence in the human body, and its interaction with matter.

It may appear to some that I am beating a dead horse in thus criticizing at length the doctrine of the atomic self. It is held in certain quarters that the notion of *substratum* has been so thoroughly exploded that it is scarcely necessary to waste time over it. Whatever the self may be, it is said, we can at least be sure that it is not the Lockian *substance*, for it is mere misconception to assume that things have an indefinite and unintelligible core of this kind. But it is by no means evident that the doctrine is so dead as those who speak thus would have us believe. No doctrine can hold its own for centuries as the orthodox belief of the scholarly world, without leaving its trace upon the thought even of an age more or less influenced by new ideas. The doctrine of the atomic self is emphatically that of the plain man to-day, *i. e.*, it embodies the notions cherished by vastly the greater part of the cultivated persons whom one meets, touching the nature of the mind and its connection with the body. Until quite recently it was about the only doctrine taught to the youth in the higher institutions of learning in England and America, and it is still presented as the final word of wisdom in many quarters where one might have expected to find something better. Nor must it be overlooked—and this is a point of especial importance—that some of those who appear to be the most energetic in their repudiation of the atomic self do not really repudiate it at all. They refine it away, they sublimate it, they deny to it a place in time as well as a position in space, they render it the most incomprehensible of all incomprehensibles, they call it a self-activity—and, in the face of all this, they go on thinking of it indefinitely in much the same way as the plain man thinks of his atomic self. The dust of words which they have raised makes it more or less difficult to distinguish what is the true content of their doctrine. Nevertheless, a careful examination cannot fail to reveal that they are true descendants of the substratumists, and that, if their balloon has taken an all too erratic flight into the region of thin air, it

is only because they have been more incautious than the genuine substratumist in throwing out the materialistic ballast that keeps the doctrine of the atomic self from resolving itself into mere negations. Of this Neo-Kantian branch of the substratumists I have treated elsewhere,[1] and it is unnecessary for me to enter into the matter here at greater length.

But what shall we say to one who drops the substratum self altogether and assigns to *ideas* the rôle which has heretofore been assigned to it—who makes ideas determinative of motions in matter? This can hardly be said to be a doctrine affected by the plain man, for he must have, as we have seen, a something in which ideas may inhere or to which they may in some sense belong. Still it is a possible doctrine, and it may not without justice be regarded as a development from or a modification of the plain man's doctrine. That they have much in common becomes evident just as soon as we endeavor to make quite clear what is meant by the statement that ideas are determinative of motions in matter. We are to conceive that a detailed knowledge of all the motions of all the atoms constituting the body of the boy who is chasing the dog would reveal that we are not dealing with a perfect mechanism. At some point there is a break. All the motions which have preceded will not account for all the motions that follow. We must fill up this gap with ideas and suppose them to be capable of being affected by the machine and, in turn, of affecting it. In other words, the ideas become, at least for the time being, a part of the machine.

Now, that ideas should become even for an instant a part of the machine can seem simple and natural only to one who has no clear conception of all that this implies. If the statement that matter can act upon ideas and ideas upon matter is to mean anything at all, and is not to remain an empty collocation of sounds, we must conceive the ideas to be *present* in the body. The machine needs patching up *at the break*, and the insertion of a coupling which is not present is manifest nonsense. If the ideas are not entities which exist in space, if they are nowhere, then they are, of course, no nearer to the point at which they are needed than they are to any other point in the body. Indeed,

[1] THE PSYCHOLOGICAL REVIEW, January, 1897.

they are no nearer to this point than they are to any point in any other body, and the notion of the insertion of ideas to fill a gap simply lapses. Descartes realized this truth perfectly well, and he took care to put his soul in the little pineal gland, where it could do the most good. If we deny that the things which interact are *present* to each other, if we deny that they form part of the same system in space, we exenterate our notion of interaction, and it becomes a mere shell. As a matter of fact we do not have to go far afield to discover that those who trace the series of changes which run from the periphery of the body along the afferent nerves, and the series of changes which run from the central nervous system along the efferent nerves, and find it impossible to connect these with each other except with a coupling of ideas—we do not, I say, have to go far to find that these vaguely assign to ideas a spatial presence, and put them *between* the two sets of changes. They do precisely what the plain man does with his atomic self, and they do it, just as he does, without a clear recognition of what it is that they are doing.[1]

[1] "If feelings are causes, of course their effects must be furtherances and checkings of internal cerebral motions, of which in themselves we are entirely without knowledge. It is probable that for years to come we shall have to infer what happens in the brain either from our feelings or from motor effects which we observe. The organ will be for us a sort of vat in which feelings and motions somehow go on stewing together, and in which innumerable things happen of which we catch but the statistical result. Why under these circumstances we should be asked to forswear the language of our childhood I cannot well imagine, especially as it is perfectly compatible with the language of physiology. The feelings can produce nothing absolutely new, they can only reinforce and inhibit reflex currents, and the original organization by physiological forces of these in paths must always be the groundwork of the psychological scheme.

"* * * The nerve-currents, coursing through the cells and fibers, must in this case be supposed strengthened by the fact of their awaking one consciousness and dampened by awaking another. *How* such reaction of the consciousness upon the currents may occur must remain at present unsolved.

"* * * Habitual actions are certain, and being in no danger of going astray from their end, need no extraneous help. In hesitant action there seem many alternative possibilities of final nervous discharge. The feeling awakened by the nascent excitement of each alternative nerve-tract seems by its attractive or repulsive quality to determine whether the excitement shall abort or shall become complete. Where indecision is great, as before a dangerous leap, consciousness is agonizingly intense. Feeling, from this point of view, may be likened to a cross-section of the chain of nervous discharge, ascertaining the links already laid down, and groping among the fresh ends presented to it for the one which seems best to fit the case."—James, 'Psychology,' Chapter V.

If, then, the ideas are to be built into the machine in even a semi-intelligible sense, they must be conceived to be present in the body. We have seen above that, when we strive to get a clear understanding of the nature of the presence of the atomic self in the body, we discover it to be a dimly-imagined material presence. Here the case is the same. But this vague attribution to ideas of a material presence must go the way of all misconceptions when its true significance is brought to light. Let us suppose that the idea thus made determinative of motions in matter is that of a yellow dog. Shall we place this at a definite point in the mist of moving atoms that constitute the boy's brain? Can atoms move toward it and away from it? Can they touch it? Can it move from place to place? Is it spread out in space as it seems to the boy to be, or must we assume it to be a mathematical point? If it cannot lie between two atoms, approach and be approached, touch and be touched, in what sense can it be declared to be present? He who talks vaguely of its presence, and does not raise any of these questions, is walking in thick darkness and is unaware of that fact. He dimly conceives ideas to be material, just as the plain man dimly conceives of the atomic self as material. He puts them in space, and yet he would shrink from the consequences that this entails, did he realize what those consequences are.

This doctrine that ideas may be used to patch up a defective mechanism does not need to be discussed at great length, because it differs so little, in any point that need concern us here, from the doctrine of the atomic self. One is impressed, in studying both the original doctrine and its modification, with the thought that it is exceedingly hard for the human mind to shake itself free from materialistic ways of thinking. Some of those who have been most anxious not to be accounted materialists have retained the most unmistakable traces of materialistic thought.

EXPERIMENTAL INVESTIGATIONS CONCERNING THE DEPTH OF SLEEP.[1]

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The question of the depth of sleep has already been discussed in a book by one of the present writers[2] from both the critical and the experimental standpoints. The curves of depth constructed by Kohlschütter, Mönninghoff and Piesbergen, Michelson, and Lambranzi were there given, and the methods used by these and other investigators criticized.[3]

In general, auditory stimuli were used as means of wakening; but Michelson and Czerny experimented with electricity also, and Lambranzi with auditory, visual, and olfactory stimuli, used one at a time or simultaneously. In the above-mentioned book one of us described some experiments performed with tactile and pressure stimuli, using Griessbach's esthesiometer with sharp and blunt points.

This method had the great advantage of producing in the sleeper (at least where normal subjects were concerned) a single and continuous excitation, instead of a series of excitations separated by a more or less extended interval of time. Note was made not only of the degree of pressure at which waking was effected, but also of the degree at which the sleeper made defensive or withdrawing movements; so that, constructing a diagram with the values obtained, two curves were obtained: one of conscious reaction, representing the curve of the points of complete awaking, or *curve of the depth of sleep*, the other the curve of *subconscious reaction*. Some figures were there given[4] which it is deemed advisable to reproduce here.

[1] Translated from the authors' MS. by Professor Howard C. Warren, Princeton University.

[2] S. De Sanctis: 'Die Träume,' etc. Durch zahlreiche Nachträge des Verfassers erweiterte Uebersetzung, von O. Schmidt. Halle, Marhold, 1901, pp. 207 ff.

[3] For bibliography, see the work cited.

[4] S. De Sanctis, op. cit., p. 214.

EXPERIMENTS ON A BOY OF EIGHT YEARS; DURATION OF SLEEP, FROM 9 P. M. TO 8 A. M.

SERIES 1.

Hour	Subconscious reaction	Waking-point
10	15	60
11	35	70
4	10	25
7	10	30

SERIES 2.

Hour	Subconscious reaction	Waking-point
10	15	60
11	30	60
4	15	30
7	15	25

SERIES 3.

Hour	Subconscious reaction	Waking-point
10	15	65
11	35	65
4	15	35
7	10	40

These tests show that in the child experimented upon the depth of sleep was greatest at 11 p. m. and became lighter in the early hours of the morning. They also show that the subconscious reaction maintained approximately the same proportion to the degree of depth of sleep throughout.

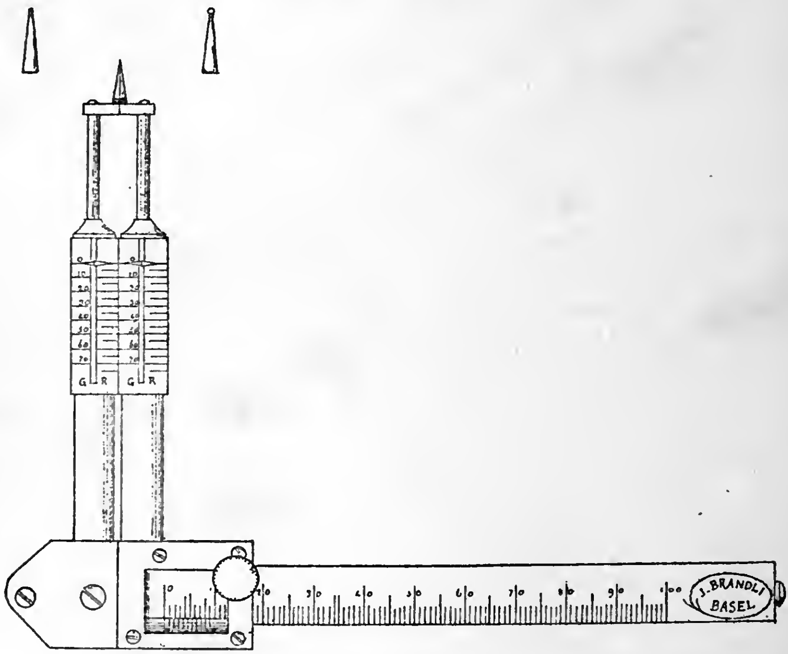
The experiments referred to were meager, however, and hence far from conclusive. We desired to carry them out on a much larger scale and to attempt by means of the method of tactile excitations to construct the *curve of the depth of sleep* for various subjects.

The instrument employed was the *Griessbach esthesiometer* (made by Brändli of Basel), adapted to admit of using stimuli given by a single point. For this purpose a metal cap was fitted over the two points of the instrument brought close together, but in such a way that the action of the moving shafts was perfectly free; the extremity of the cap terminated in either a rounded surface or a blunt point. (See Fig. 1.)

We were soon compelled to discard the rounded point, for the reason that the stimulation was generally insufficient to produce awakening when the depth of sleep had attained a certain intensity; we therefore made use of the blunt point, or of the two sharp points of the esthesiometer brought together so as to

form a single point. However, as basis of experiment with all subjects the blunt point was chiefly employed, as it best answered our purpose, being able to bring about awakening without pain, and at the same time doing away with the danger of the instrument's slipping and inflicting injury, in the event of a sudden movement by the sleeper. Moreover, in cases of light sleep, if the awakening were effected suddenly by means of a painful stimulus, it would not have been possible to construct an exact curve, especially for the periods during which the depth of sleep was at a minimum.

FIG. 1.



The experiments were carried on for about six consecutive months, one or at most two per night, so that the normal course of sleep might not be in any way altered, as would evidently have been the case had the tests been made too close together. The experiments were always performed at different hours on successive and irregularly alternated nights; several observations were made in hours already used, in order that a more exact value might be obtained, and at least four different periods

were selected for each hour of sleep, so that we might find the waking-points for every ten or fifteen minutes, in order to make the curve as complete as possible.

In our notebook we recorded all the observations as they were made, and any influences which might have effected a modification of the physical or moral well-being of the subject of experiment.

The four normal subjects upon whom experiments were made were relatives of one of the writers; two were of mature age, the other two young people; all were sound and in the best of health. They slept in separate rooms. One of us, taking his station in an intermediate room, could keep close watch of the moment at which they fell asleep, determining it from the cessation of movement, the rhythm of breathing, etc. We had previous knowledge of the habits of each of the subjects in falling asleep.

The hour at which they fell asleep being thus ascertained, the experiment was performed at the time selected. One of us entered in bare feet and without making the slightest noise approached the sleeper's bed, which had beforehand been moved out from the wall to enable him to move around it easily. The head of the bed was low, so as to present no obstacle to his movements. Having ascertained that the subject gave no sign of waking, he threw a dim light upon his forehead by means of a small dark-lantern, provided with a screen below and having the luminous aperture reduced to a minimum, so that the light should not strike the subject's eyes at all, but illuminate only the part under examination and the scale of the esthesiometer. Then the point of the instrument, held vertically in the right hand, was placed gently on the forehead, in contact with the left frontal protuberance at its upper extremity near the borders of the hairy part of the scalp. (The forehead was selected to avoid the necessity of uncovering the sleeper, the left side in order that the stimulation might always be at the same point and because it was more accessible, owing to the general custom of sleeping on the right side.) Pressing the instrument down in a uniform and continuous manner, so that the spring passed along the graduated scale from end to end in twenty seconds, the ob-

server watched carefully for the instant when the sleeper first made a movement—a wrinkling of the forehead, slight movement of the head or limbs, change in the rhythm of respiration, etc.; he then read on the scale the number opposite the point where the indicating needle was situated at that instant; the pressure was continued without interruption until the subject awoke, and note was made of the point at which the waking occurred.

If, as occasionally happened, the reflex movements of the sleeper were somewhat energetic, such as a sudden movement of the head or a change of position of the body, so that it was impossible to keep the instrument applied continuously to the forehead, it was withdrawn and the excitation recommenced after an interval of ten seconds, in such a way that the pause and the duration of the new excitation should together occupy not more than thirty seconds.

The experiments on the abnormal subjects were made in the months of March and April, 1901. There were five such subjects: two epileptics of long standing (E. B., T. R.), one case of epilepsy due to a wound (G. G.), one hystero-epileptic degenerate (F. B.), and one case of paralytic dementia (L. M.).

These tests also were made on irregularly alternated days (the subjects never knowing whether they would be wakened in the night or not) and at various hours, a couple of times per night with an interval of three or four hours between the two tests.

On account of the many practical difficulties not more than 25 or 30 esthesiometric readings per subject could be made, on the average, giving the waking-points for every twenty minutes, or thirty at the most; these points, however, were more than sufficient to give us an amply complete curve, the variations in the depth of sleep being quite large, as we were convinced from the experiments on the normal subjects.

On the nights of experiment one of us, taking his station in a room near that in which the patients slept (at the Psychiatrical Clinic of the University of Rome), determined the time at which each of them fell asleep. The subjects under examination, not more than three of whom were used at a time, slept together in a large dormitory, but in beds situated so far apart that the experiments made on one patient did not disturb the slumber of

the others. One of us softly entered the dormitory, and standing behind the bed, which was placed at a distance from the wall, cast the light of his lantern, as before, on the forehead only, and pressed on this part with the instrument, always upon the left frontal protuberance, observing and writing down at once the degrees corresponding to the first reflex reaction and the complete awaking of the sleeper—proceeding, in a word, as with the normal subjects.

The observations made each time, together with the data obtained from questions put on the morning following each night of experiments, were recorded in a notebook in which was also recorded anything which might have happened during the preceding day to any of the patients experimented upon.

The following tables give the values obtained in the particular experiments for each of the normal subjects. It is to be observed that for the first two normal subjects the figures represent the average of tests repeated several times, for the sake of verification, in each particular hour of sleep and as frequently as possible.

The first column indicates the hour of sleep at which the experiment was made, the second gives the value corresponding to the first *subconscious reaction*, the third, that corresponding to the *waking-point*.

Cases were found where the entire range of the esthesiometer was insufficient to produce awakening. In such cases it was necessary to bring a second excitation to bear upon the subject; that is, it was necessary to recommence the pressure and continue it until, as a result of the summation of excitations, the awakening was effected. Similarly, it sometimes happened that the regular course of the esthesiometer was interrupted by a vigorous (unconscious) withdrawing movement on the part of the sleeper; it was then necessary, as in the other cases, to recommence the pressure after a short pause and proceed with it till he awoke. The former case is indicated in the third column by numbers joined by a plus sign (+), the latter by figures separated by a comma. Finally, in the fourth column are placed the data respecting dreams obtained from questions put at the time of the experiment, that is, as soon as

the subject was awake; affirmative answers are indicated by a plus sign (+), negative by a minus sign (—), doubt by an interrogation point (?).

From these experiments, by joining the several waking-points, we were able to obtain a *curve of the depth of sleep*. With this curve we give another, which shows the manner in which the *subconscious reaction* varies; and we might easily have added still a third, the *curve of dreams*, had it not seemed to us somewhat inappropriate to place numerical data and data gathered from the putting of questions in one and the same graphic representation.

Normal subject O. N., male, aged 15; rather delicate constitution, but healthy. Temperament excitable, somewhat impulsive. Sleep usually deep and regular. Seldom wakes up in the night. Is accustomed to fall asleep soon after going to bed. Is not a good dreamer; always remembers his dreams, which are, however, for the most part trivial.

Table I.—Normal Subject O. N., Male.

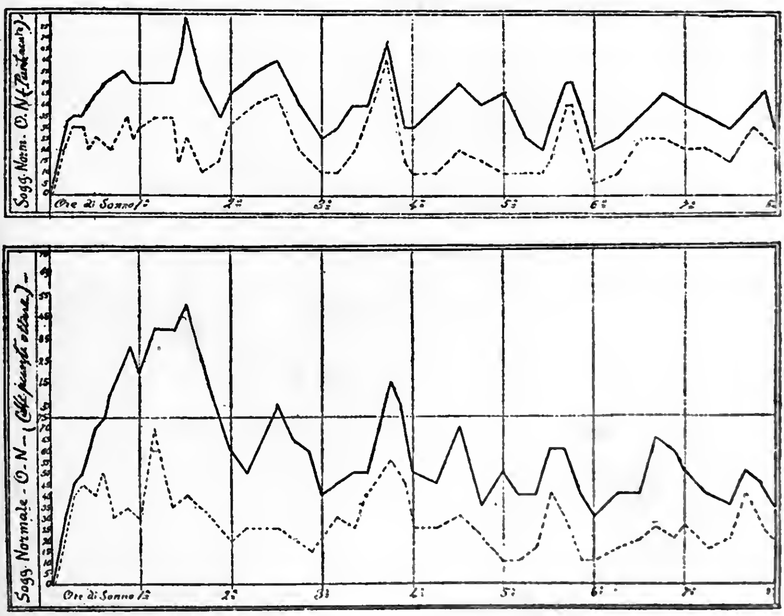
EXPERIMENTS WITH SHARP POINT OF THE ESTHESIOMETER.

Hour of Sleep.	Subcon. Reaction.	Waking Point.	Dreams.	Hour of Sleep.	Subcon. Reaction.	Waking Point.	Dreams.
0.10	20	30	—	3.40	60	65	+
0.15	30	35	—	3.50	15	30	?
0.20	30	35	—	4	10	30	—
0.25	20	40	—	4.15	10	40	—
0.30	25	45	—	4.30	20	50	—
0.40	20	50	—	4.45	15	40	—
0.50	35	55	—	5	10	45	+
0.55	25	50	—	5.15	10	25	+
1	30	50	—	5.25	10	20	+
1.10	35	50	—	5.30	15	30	?
1.20	35	50	—	5.40	40	50	+
1.25	15	65	—	5.45	40	50	?
1.30	25	25, 55	—	5.55	10	30	+
1.40	10	50	—	6	5	20	?
1.50	15	35	+	6.15	10	25	+
2	30	45	—	6.30	25	35	?
2.15	40	55	?	6.45	25	45	+
2.30	45	60	+	7	20	40	+
2.45	20	40	—	7.15	20	35	+
3	10	25	?	7.30	15	30	+
3.10	10	30	—	7.45	30	40	—
3.20	20	40	—	7.50	25	45	—
3.30	35	40	+	8	20	30	—
				8.15	20	30	+

TABLE II.—NORMAL SUBJECT O. N. (THE SAME).
EXPERIMENTS WITH BLUNT POINT OF THE ESTHESIOMETER.

Hour of Sleep.	Subcon. Reaction.	Waking Point.	Dreams.	Hour of Sleep.	Subcon. Reaction.	Waking Point.	Dreams.
0.10	20	30	—	4	25	50	?
0.15	40	45	—	4.15	25	45	—
0.20	45	50	—	4.30	30	70	?
0.30	40	70	—	4.45	20	35	+
0.35	50	75	—	5	10	50	+
0.40	30	30, 55	—	5.10	10	40	+
0.50	35	35, 20, 50	—	5.20	15	40	—
1	30	30, 20, 45	—	5.30	40	60	+
1.10	70	70, 30, 15	—	5.40	30	60	+
1.20	35	35, 45, 40	—	5.50	10	40	+
1.30	40	40, 30, 20, 35	—	6	10	30	+
1.45	30	75 + 15	—	6.15	15	40	+
2	20	60	—	6.30	20	40	+
2.10	25	50	?	6.40	25	65	+
2.30	25	25, 35, 20	+	6.50	20	60	—
2.40	20	20, 45	—	7	25	50	+
2.50	15	60	—	7.15	15	40	—
3	20	40	—	7.30	20	35	—
3.10	30	45	—	7.40	40	50	+
3.20	25	50	+	7.50	25	45	—
3.30	40	50	+	8	20	35	+
3.45	55	55, 20, 15	+	8.10	25	30	—
3.50	45	45, 25, 10	—	8.20	35	50	+

FIG. 2.



Note in subject O.N. that the maximum depth is reached in the first half of the second hour. Note also the infrequency of dreams in the first half and their relative frequency in the second half of the sleep period. (Compare Fig. 2.)

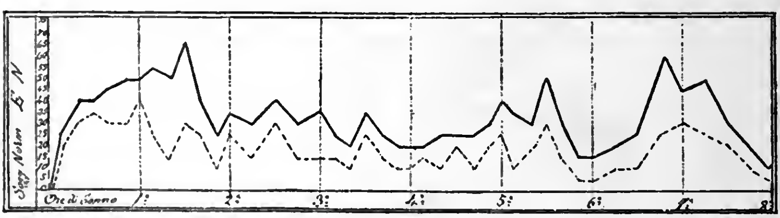
Normal subject E. N., female, aged 21; good constitution, at the time slightly debilitated from study, somewhat emotional; intellect and memory very good. Sleeps rather lightly; wakes up easily during the night; ordinarily falls asleep a considerable time after going to bed. Has frequent dreams and remembers them well.

TABLE III.—NORMAL SUBJECT E. N., FEMALE.

Hour of Sleep.	Subcon. Reaction.	Waking Point.	Dreams.	Hour of Sleep.	Subcon. Reaction.	Waking Point.	Dreams.
0.10	20	25	—	4.10	15	20	—
0.20	30	40	—	4.20	10	25	+
0.30	35	40	+	4.30	20	25	+
0.40	30	45	—	4.40	10	25	+
0.50	30	50	?	4.50	20	30	+
1	40	50	—	5	25	40	+
1.10	25	55	+	5.10	10	35	+
1.20	15	50	—	5.20	20	30	+
1.30	30	65	?	5.30	30	50	+
1.40	25	30	+	5.40	15	30	+
1.50	10	25	+	5.50	5	15	+
2	25	35	+	6	5	15	+
2.15	15	30	+	6.15	10	20	+
2.30	30	40	+	6.30	10	25	+
2.45	15	30	?	6.45	25	55	+
3	15	35	?	7	30	45	+
3.10	15	25	+	7.15	25	50	+
3.20	10	20	+	7.30	20	30	+
3.30	25	35	+	7.45	10	20	+
3.40	15	25	+	8	5	10	+
3.50	10	20	+	8.15	10	15	+
4	10	20	+	8.30	10	15	—
				8.45	5	10	+

Note in normal subject E. N. that the depth of sleep attains its maximum after an hour and a half of sleep; that in the period between six and a half and seven and a quarter hours the depth is considerable. Dreams vary at first and become frequent in the succeeding hours until morning. (Compare Fig. 3.)

FIG. 3.



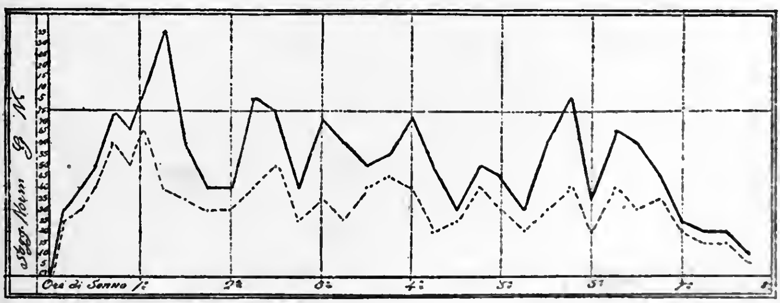
Normal subject G. N., male, aged 60; very robust constitution, mentally normal. Sleeps soundly during the early part of the night. In the latter part of the night wakes easily. Falls asleep soon after going to bed. Is a good dreamer and remembers his dreams fairly well.

TABLE IV.—NORMAL SUBJECT G. N., MALE.

Hour of Sleep.	Subcon. Reaction.	Waking Point.	Dreams.	Hour of Sleep.	Subcon. Reaction.	Waking Point.	Dreams.
0.10	25	30	—	4	40	70	+
0.20	30	40	?	4.15	20	50	+
0.30	40	50	+	4.30	45	30	+
0.40	60	70	+	4.45	40	50	+
0.50	50	65	+	5	30	45	+
1	65	65, 15	?	5.15	20	30	+
1.15	40	40, 60, 10	+	5.30	30	60	+
1.30	35	60	+	5.45	40	40, 40	+
1.45	30	40	—	6	20	35	+
2	30	40	+	6.15	40	65	+
2.15	40	40, 40	—	6.30	30	60	+
2.30	50	75	+	6.45	35	55	—
2.45	25	40	+	7	20	25	—
3	35	35, 35	+	7.15	15	20	+
3.15	25	60	+	7.30	15	20	+
3.30	40	50	+	7.45	10	15	—
3.45	45	55	+				

Note in normal subject G. N. that the maximum depth is attained at the beginning of the second hour; that it is quite marked also in the period between five and a half and six and a half hours. Dreams were noted in almost every experiment. (Compare Fig. 4.)

FIG. 4.



Normal subject An. N., female, aged 55; robust physical constitution. Quiet temperament. Sensibility and intellect normal. Is accustomed to fall asleep soon after going to bed. Generally sleeps well; her sleep does not ordinarily extend

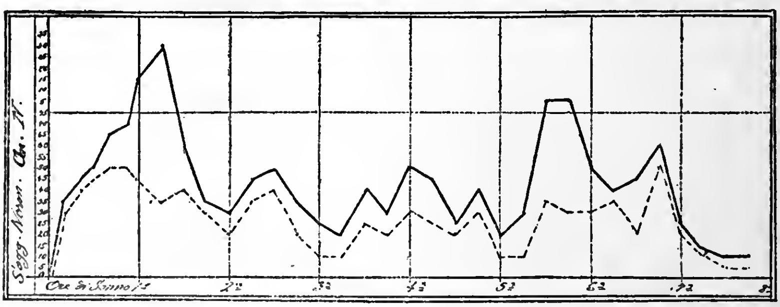
beyond eight hours. Dreams frequently and has a good memory for dreams.

TABLE V.—NORMAL SUBJECT AN. N., FEMALE.

Hour of Sleep.	Subcon. Reaction.	Waking Point.	Dreams.	Hour of Sleep.	Subcon. Reaction.	Waking Point.	Dreams.
0.10	30	35	+	4	30	50	+
0.20	40	45	+	4.15	25	45	+
0.30	45	50	+	4.30	20	25	+
0.40	50	65	+	4.45	30	40	+
0.50	50	70	?	5	10	20	+
1	45	45, 45	+	5.15	10	30	+
1.15	35	35, 40, 30	+	5.30	35	35, 45	+
1.30	40	60	—	5.45	30	30, 50	+
1.45	30	35	+	6	30	50	+
2	20	30	+	6.15	35	40	+
2.15	35	45	—	6.30	20	45	+
2.30	40	50	+	6.45	50	60	+
2.45	20	35	+	7	20	25	+
3	10	25	+	7.15	10	15	+
3.15	10	20	+	7.30	5	10	+
3.30	25	40	+	7.45	5	10	—
3 45	20	30	+				

Note in normal subject An. N. that the maximum depth is reached at the beginning of the second hour of sleep, and that it is also considerable in the sixth and seventh hours. Dreams are very frequent; they occur even when the depth of sleep is at its maximum. (Compare Fig. 5.)

FIG. 5.



The tables dealing with the measure of sleep in the five pathological subjects are very complex, since it seemed desirable to note (1) not only the moment at which the first reflex movement on the tactile-pressure excitation appeared, but also the nature and extent of such movement; (2) the phenomena accompanying complete awaking; (3) recollection of dreams

and, where recalled, their content; (4) convulsions and any other pathological phenomena observable in the subject on the day preceding the night of experiment, etc.; besides many other matters which are of no direct interest to the readers of this REVIEW.

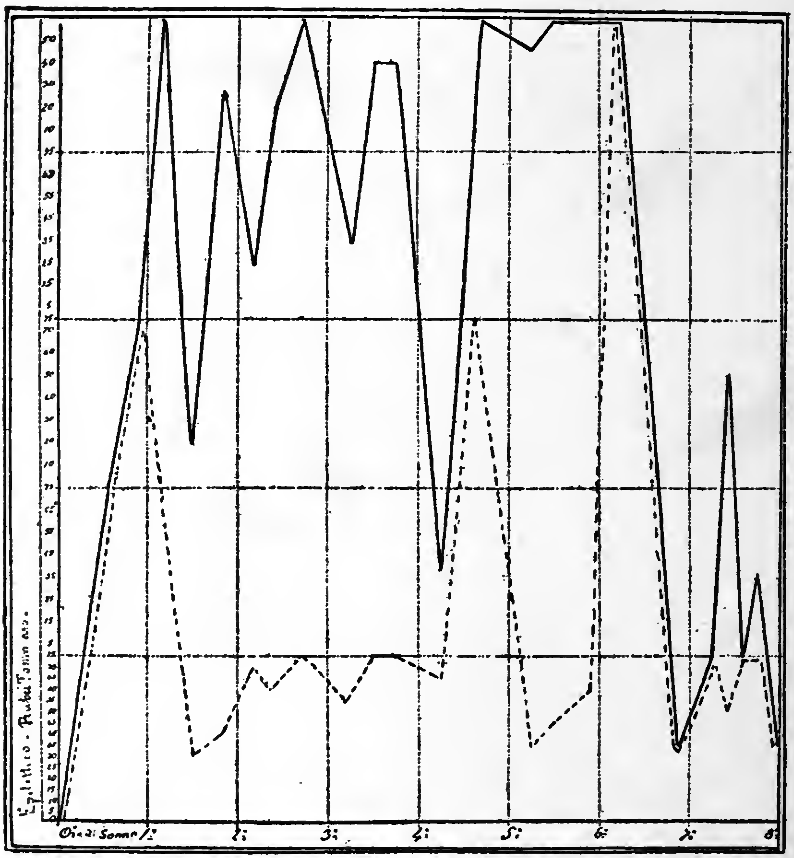
For this reason we shall make a descriptive report of the observations without giving tables, and shall reproduce here the graphic representation only, showing the curves of the depth of sleep and subconscious reaction.

Tommaso Rubei, aged 33. In childhood he had typhoid; was early given to self-abuse. About his eighth year he began to have attacks of vertigo, but without loss of consciousness. At the age of 15 he experienced a great fright, and two years later began to have convulsions, which at first were very frequent and closely connected with atmospheric changes, and which gradually became rarer. For several years they have appeared every 20, 30 or 40 days; but attacks of petit mal and vertigo are also not infrequent. These attacks occur almost without exception at night and are sometimes preceded by a feeling of tightening at the throat; the attack consists of tonic and clonic convulsions lasting from 5 to 8 minutes followed by coma and deep sleep. Movements and sensibility are normal, secretions of sweat abundant and easily provoked; the pupillary reaction is slightly tardy; the knee-jerk is more pronounced on the left side, while the epigastric reflex appears only on the right. His demeanor is ordinarily quiet, temperament somewhat depressed. Outbursts of anger are frequent, memory somewhat feeble. His intellectual gifts are slight.

Rubei usually sleeps well and without waking up in the course of the night; his sleep is quiet and sound. According to patient's report he dreams every night, generally of pleasant things, sometimes about matters connected with his daily life, members of his family, etc. Formerly he was often troubled with erotic dreams, followed by pollution. He does not remember whether previous to his illness his sleep exhibited any phenomena of special interest. Ordinarily he does not notice any alteration in sleep before an attack; but in the past he sometimes had terrifying dreams.

His recollection of dreams generally varies much, especially with atmospheric conditions (?); he declares that he can not recollect them at all on mornings after wet or rainy days. At other times, he says, he remembers them very well and to the minutest particulars, but he is seldom able to relate them with any great clearness.

Fig. 6.



See Fig. 6. The curve shows that Rubei's sleep is of extraordinary depth, and that it continues deep with few oscillations till he awakes.

The line representing subconscious reaction shows great differences of proportion with the curve of depth in the third and fourth hours of sleep.

It is a noteworthy fact that to awaken him it is almost always necessary to employ a number of stimuli and to push them to the maximum several times, while in the other subjects this is only exceptionally the case. Moreover, the subconscious reactions were for the most part feeble and entirely out of proportion to the intensity of the stimulus—slight movements of the head, more or less marked wrinkling of the brow, sometimes movements of the hand, as if to brush the stimulus away from the head. Awakening did not occur generally till several seconds after the last stimulation; the subject was unable to state the number of times he had been touched, but several times he declared that he had been aware of a vague feeling of discomfort on the forehead. He usually fell asleep again a few minutes after the awakening was accomplished.

In contrast with the other patients, who generally fall asleep soon after going to bed, Rubei usually remains awake for half an hour or longer. As regards the effect of the epileptic attacks upon his sleep little can be said, inasmuch as in the month of experiment they occurred but three times during the night; from experiments made March 11, 1901, at 10:35 p. m., twenty minutes after an attack (after 1 h. 30 m. of sleep), and March 22, 1901, at 1 a. m., fifteen minutes after an attack (after 4 h. 15 m. of sleep), it appears that the waking-point is lowered in comparison with the same hour on other nights without attacks. On the other hand, the waking-point remained very high when the experiment was made 1 h. 25 m. after the attack (March 23, 1901, after 4 h. 45 m. of sleep).

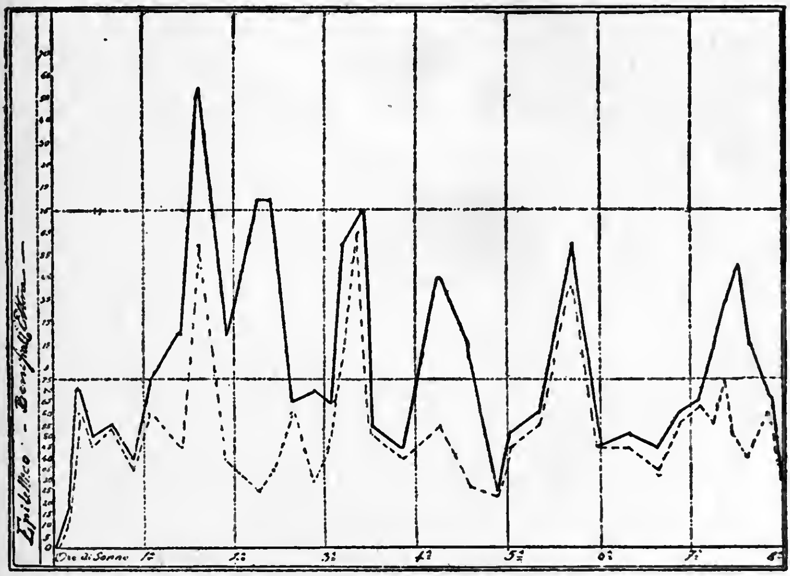
From questions put to him regarding his dreams it appeared that, even when Rubei declared that he dreamed, he was unable for the most part to relate at all in detail the subject of the dream. Altogether, in a total of 28 experiments, twice he was able to relate his dream, 9 times he recalled the subject in a general way, always as 'pleasant' in character, 7 times he did not dream at all, and 10 times he was uncertain whether he had dreamed or not. The strangest feature is that the two times when he recollected the dream fairly well were in experiments following close upon the beginning of sleep. Of the other dreams, three occurred in the first four, and six in the last four hours of sleep.

Ettore Benefialle, aged 24. In childhood he was subject to rather frequent convulsions, the attacks at times lasting throughout the day. Later they became less frequent, but were supplemented by attacks of petit mal (vertigo and sudden loss of consciousness). At present the severe chronic attacks occur very seldom; on the other hand, mild attacks are more or less frequent. The attack is preceded by an aura consisting of a tingling sensation in the right hand with clouding of vision. It lasts 4 or 5 minutes with tonic and clonic shocks, more powerful on the right side than on the left, followed by a state of coma; there is generally no frothing at the mouth. The mild attacks consist in a clouding of vision—'the light goes away from his eyes'—he sees, as it were, a ball of white flame, which hovers before his eyes, causing much discomfort; he feels as if his body were split in two in the middle. All this generally lasts for a few seconds. Afterwards he feels torpid, and if the attack occur at night he dare not go to sleep afterwards, 'for fear the trouble might come on again.' The attacks occur by day as well as at night, but more frequently at night. There is a slight defect of hearing, and of smell on the right side. The tactile sensibility appears to be less on the right side. Visual acuteness is also inferior, especially on the right. The knee-jerk is normal. Mental capacity inferior. Temperament liable to variations without apparent reason; B. is quiet and very devout; he is addicted to pederasty.

B. usually sleeps well; his sleep is quiet and sound; if he happens to wake in the course of the night he falls asleep again immediately; he says that he has sometimes waked up with a start as a result of terrifying dreams, crying out in fear and calling to the attendants for help. He ordinarily dreams every night, but when he has one of the severer attacks during the night he does not dream at all. In general his dreams are pleasant, sometimes they are strange and terrifying; quite frequently, especially in recent times, erotic dreams occur, which are repeated several times in succession, almost always in the same way, followed in the majority of cases by pollution, and he wakes up with a pleasant impression. He also dreams, though not often, of things that have happened in

the course of the day or in preceding days. He says that he has always dreamed, and that he observes no difference in his manner of sleeping on account of the disorder to which he is subject. When his dreams are terrifying he retains a vivid impression, and continues in fear during the day, dwelling on them often. He generally remembers his dreams well; but when attacks occur, even the milder ones, and he chances to dream, he does not remember the subject of his dreams at all.

FIG. 7.



See Fig. 7. The curve for the depth of sleep throughout its course is high as a whole, but does not reach the elevation of Rubei's. The course of the line of subconscious reaction maintains a fairly constant proportion to the curve of depth; the least constancy of proportion occurs at the beginning of the third and fifth hours of sleep. In his case also the first reaction to the stimulus was almost always slight. In waking, the return of consciousness was rapid, so that the subject was always in condition to answer pertinently the questions asked him as soon as he was awake.

During the month and more in which B. was subject to our experiments the epileptic attacks occurred but once in the classic form, and this was at night; the other times they were mild and of very short duration. In an experiment made 20 minutes after the severe attack his sleep was found to be much deeper than in the corresponding hour of other nights free from attacks (after 30 minutes of sleep). In five other experiments also made after attacks the depth was notable (after 2 h. 15 m., 3 h. 20 m., 4 h. 15 m., 5 h. 20 m., and 7 h. 15 m. of sleep), but without much deviation from the normal mean. The patient, on the other hand, asserts that his sleep is not so sound after the attacks.

From the observations made with reference to his dreams it appears that, contrary to what Benefialle asserts, namely, that he dreams every night, 22 times he did not dream at all, while 16 times he claims to have had dreams; the dreams were in general trival, sometimes erotic; his recollection of them at the time was very cursory, but on the following morning he was able to describe the dream in greater detail (*paramnesia onirica?*). The greatest frequency of dreaming occurred between the second and fourth hours of sleep; there were no dreams (except once, after 1 h. 5 m. of sleep) in the first two hours; there were very few in the latter half of the sleep period, though they were somewhat more frequent towards the end.

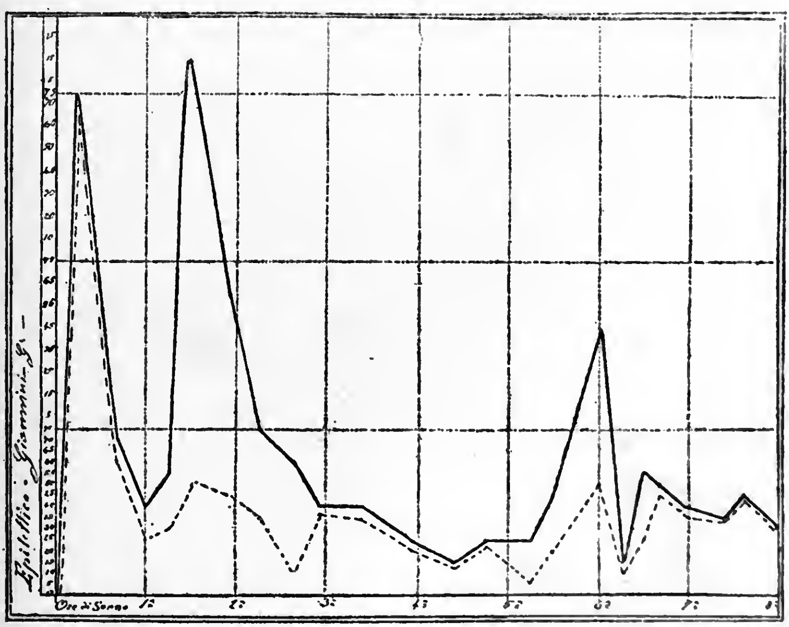
Gioacchino Giannini, aged 18. About his sixth year, in trying to avoid the wheel of a cart, he was caught between it and the guard, receiving a rather severe injury on the head; taken to the hospital, he recovered consciousness after 48 hours and did not suffer any ill-effects during the 25 days which he spent at the hospital. For seven or eight years he was perfectly able to pursue his work, when suddenly one night, without any reason for it, he was seized, while in bed, with tonic and clonic convulsions on the left side; a week later the attack was repeated in the same way, and from then on they recurred every 30 or 40 days. At the end of two years he was operated upon. Following the operation (raising of the right parietal bone which was somewhat depressed) there were no more attacks for about three months. But as the result of an injury received upon the scar the convulsions reappeared in the same

form as before, once or twice a month, and have recurred incessantly since. The patient says that he feels himself drawn sharply toward the left, he calls for help and lies on the bed or sits down so as not to injure himself during the attack; the latter is generally severe and always more violent on the left side, but of short duration, with rapid return of consciousness. In general the attacks occur by day or about evening while he is going to bed, almost never during sleep. Nothing else worthy of note.

Giannini is a sound sleeper; according to his report he seldom wakes in the course of the night; only once in 5 or 6 days do dreams occur; their content is generally of little interest; they are sometimes terrifying, occasionally erotic. He says that when he has had one of his attacks during the day he sleeps more soundly and does not dream at all. He is not aware of any differences in his sleep since he has been subject to the attacks from when he was in good health.

See Fig. 8. In this case the curve of sleep shows a course which is quite unique; it quickly reaches a very high point

FIG. 8.



within the first half hour, descending quite as rapidly in the second half hour. It rises again, reaching the maximum after an hour and a half; it then falls gradually, only to rise again to a notable height about the end of the sixth hour, thereafter maintaining itself at a medium level till the time of waking. In the majority of tests, except in the periods of greatest depth, the subconscious reactions were rather marked, even for slight stimuli, and preceded the awaking by a small margin only. Patient had but two epileptic attacks, which occurred, as usual, in the day time, and did not affect his sleep. There is little to note with respect to his dreams, Giannini not being a good dreamer, and giving affirmative answers three times only.

Luigi Moriggi, aged 51; contracted syphilis at a very early age. In June, 1899, the symptoms of the present disorder set in; pains in the head, continual somnolence during the day, neglect of his duties, foolish outlay of money, pilfering the property of others, exalted ideas of his own importance, disorders of memory and speech, etc. He entered the asylum at Rome, October 19, 1899.

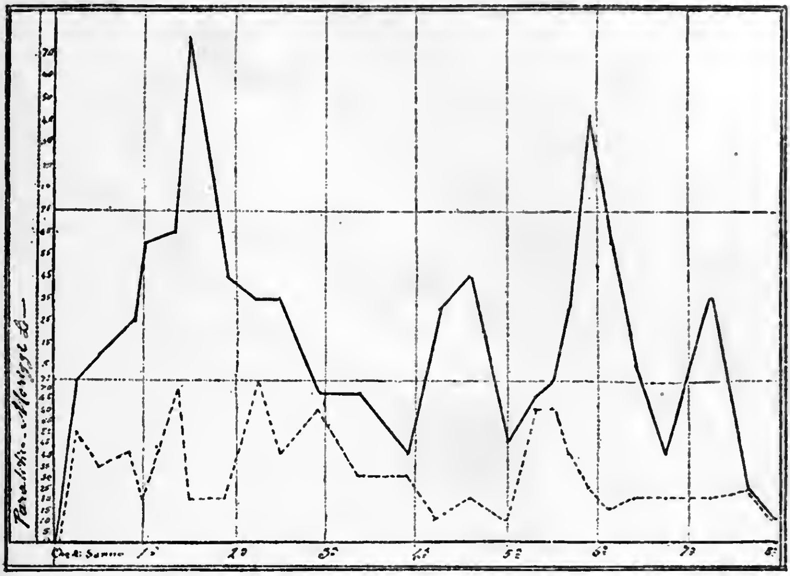
The pupils are unequal in size, reaction slow, tremors, rapid exhaustion of the facial muscles, tongue, fingers, etc.; slight indication of Romberg's sign, exaggeration of the deeper reflexes, diminution of the superficial ones, normal tactile sensibility. Demeanor apathetic, mild euphoria, wild ideas (hazy and vague) of his own greatness, affective side deficient, critical side exceedingly deficient; memory weak. Diagnosis of *dementia paralytica*.

M.'s sleep is usually light and often interrupted; he dreams but seldom, sometimes about members of his family or trivial matters. Before his illness he slept well and without interruption; he says that he dreamed more frequently then than now, but without ever being much of a dreamer. The few dreams which he has at present he does not remember at all.

See Fig. 9. The figure shows that the depth of sleep mounts rapidly at the beginning of the first hour of sleep, keeps on increasing slowly and continuously till the first half of the second hour, and then decreases with oscillations till the sixth

hour, at the end of which there is a decided rise. The depth is above the normal in this subject also.

FIG. 9.



Somewhat more variable is the line of subconscious reaction; it is conspicuously low in the second half of the sleep period, except in the fifth hour. The reactions were generally few and feeble; but before awakening was effected it was necessary to repeat the excitation at least twice. There is nothing to note regarding his dreams, Moriggi never having mentioned a single dream during the period of experimentation.

Francesco Bechelli, aged 16. His mother suffers from hystero-epileptic convulsions. He is very capricious; has always shown a perverse and unruly character, resisting his mother and sister, going even so far as to draw a knife upon them. He often runs away from home, after stealing some objects and selling them for a few *sous*, which he spends in amusement. He often sleeps in the street or a gateway, resisting the police when they endeavor to take him back home. Has several times attempted suicide. The pupillary reaction is good, the conjunctival and pharyngeal reflexes are wanting;

the knee-jerk is vigorous, other reflexes normal. There is hypesthesia of the right side of the trunk and also of the lower left limb. Amblyopia is present on the right side, hypacousia and anosmia on the left, hyperesthesia in the inguinal region. His demeanor is quiet. Decided deficiency in mental gifts, childish ideas, lack of capacity for concentration; ethical sensibility very blunt. He speaks with complacency of his own debauchery; of many of the deeds of which he is accused he has no recollection whatever, of others he admits the authorship and boasts of them. He is addicted to swearing and is exceedingly fond of indecent language. The patient has both mental and motor crises. Diagnosis of *degeneration and moral insanity*, probably *hystero-epilepsy with distinct crises.*

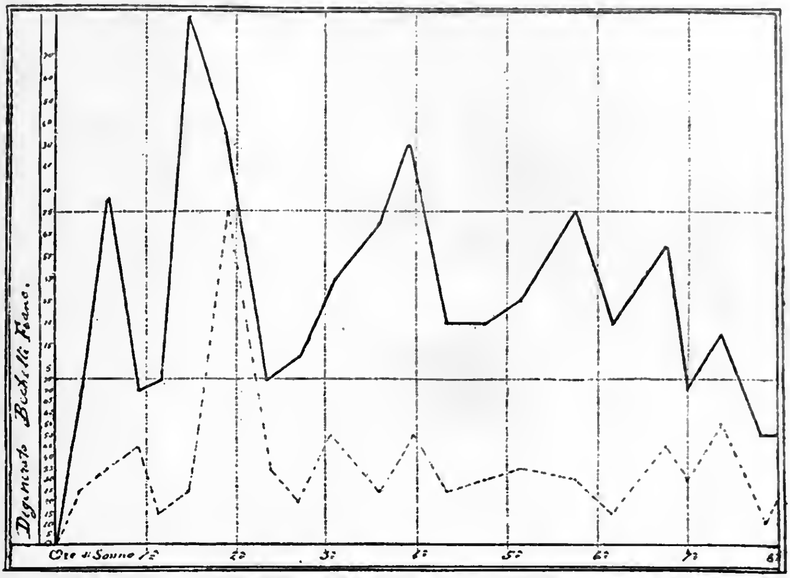
He is a sound sleeper; if he wakes up before midnight he goes to sleep again quickly, if it is near morning he remains awake and if at home gets up and goes out. For the past three or four years he has dreamed very little, previous to that more frequently; his dreams are generally pleasant, but formerly he often dreamed of being pursued by his parents seeking to beat him, or by the police, and would awake with a start. At other times he thought he saw wild beasts under the bed seeking to attack him: then he would feel, as it were, a blow on the head and wake up terrified (De Sanctis's *dream equivalent of attack*). At present, he says, when such a dream occurs he wakes up quickly, realizing that it is a dream, while if the dream is pleasant he remains asleep. His sleep has never shown any notable changes in connection with his disorder, except that, as mentioned above, dreams occur much less frequently, and that without being especially impressed by them he generally remembers them well.

See Fig. 10. His sleep increases rapidly in intensity during the first half hour; the curve attains its highest level in the second hour; then it descends, only to rise again and remain at a high level, but with oscillations which continually diminish in height till the time of waking, without exhibiting any noteworthy elevation in the second half of the sleep period.

The curve of subconscious reaction is seen to be less variable; except for a marked elevation in the second hour of

sleep it remains at rather a low level, with few oscillations. The reactions are somewhat energetic, but notwithstanding this there is no clear sign of waking until after repeated stimuli which, in the mean, if taken separately, would not reach a high degree of intensity.

FIG. 10.



His dream activity is very slight; only four times was an affirmative answer given—in experiments made after 2 h. 20 m., 5 h. 45 m., 8 h. 10 m., and 8 h. 45 m. of sleep—that is, once in the first and three times in the second half of the sleep period.

During the time when Bechelli was subject to experiment nothing was observed on either the mental or the motor side which seemed to exert a real influence on the course of his sleep.

Comparing the curves obtained from our normal subjects with the curves given by the authors cited at the beginning of this article, some rather notable differences appear at once.

As we have seen, the maximum depth falls, according to Kohlschütter, within the first hour of sleep, according to Mönninghoff and Piesbergen in the third quarter of the second hour,

according to Michelson at the end of the first, and about the same according to Czerny; while according to our observations it falls within the first half of the second hour. This would be in accord rather with the observations of Lambranzi. The diversity of results should be attributed, however, not so much to method as to individual factors, the authors mentioned having found that the point is sometimes earlier, sometimes later, according to the subject.

But where the curve of depth of sleep obtained by our method differs most notably from the others is in its later course; for while according to Kohlschütter and Michelson it descends continuously and rapidly till the time of waking, and according to Mönninghoff and Piesbergen, Czerny, and more especially Lambranzi it exhibits a marked elevation in the second half of the sleep period, according to our observations, on the contrary, the curve, although following in general a descending course, does not have a uniform path, but exhibits marked oscillations, with a maximum and minimum for each hour of sleep. The *secondary rise* of the curve appears clearly in three subjects and lasts for an hour and a half; it occurs earliest in the subjects G. N. and An. N., who are accustomed to sleep from 7 to 8 hours at the most, and later in the subject E. N., who habitually sleeps longer. The fact that we did not discover any such rise in the subject O. N., who is not much of a dreamer, appears to confirm Lambranzi's hypothesis, according to which the secondary rise is due to a real increase in the depth of sleep in connection with greater dream activity. But we shall see presently that Lambranzi's hypothesis seems to be contradicted by other facts. It appears evident, then, that each subject has a curve peculiar to himself, which differs both in elevation and in course from the others. That the depth of sleep is not the same in all individuals has been known, indeed, from the earliest times; age, sex, constitution, state of health, etc., are all factors which may cause it to vary, apart from factors extraneous to the organism which may act for a longer or shorter time on the sleeper.

Analysis of the curves of the abnormal subjects brings out noteworthy points regarding the general course of the depth of sleep and the intensity to which it attains.

The fact which stands out first of all is that in all five of the pathological subjects the depth of sleep is far greater than among the normal subjects. In the epileptic *Rubei* sleep is extraordinarily deep; in the two other epileptics, *Benefialle* and *Giannini*, although deep, it does not reach the level found in Rubei's case. In the paralytic *Moriggi* and the degenerate *Bechelli* sleep maintains throughout its course a depth at least double that of the normal cases.

As regards the epileptics, these researches completely confirm some investigations made by one of us, who found that, contrary to the assertions of most authors that all neuropaths, including epileptics, have a restless and light sleep, as a matter of fact sleep was very deep in the majority (60 per cent.) of the cases of epilepsy with the classic symptoms. In the subject *Rubei*, moreover, confirmation is found of another fact also noted by one of us, that with increase in age of the patient and length of standing of the disorder sleep becomes much deeper. As a matter of fact, Rubei is the oldest of the three epileptics and has been the longest time an invalid.[1]

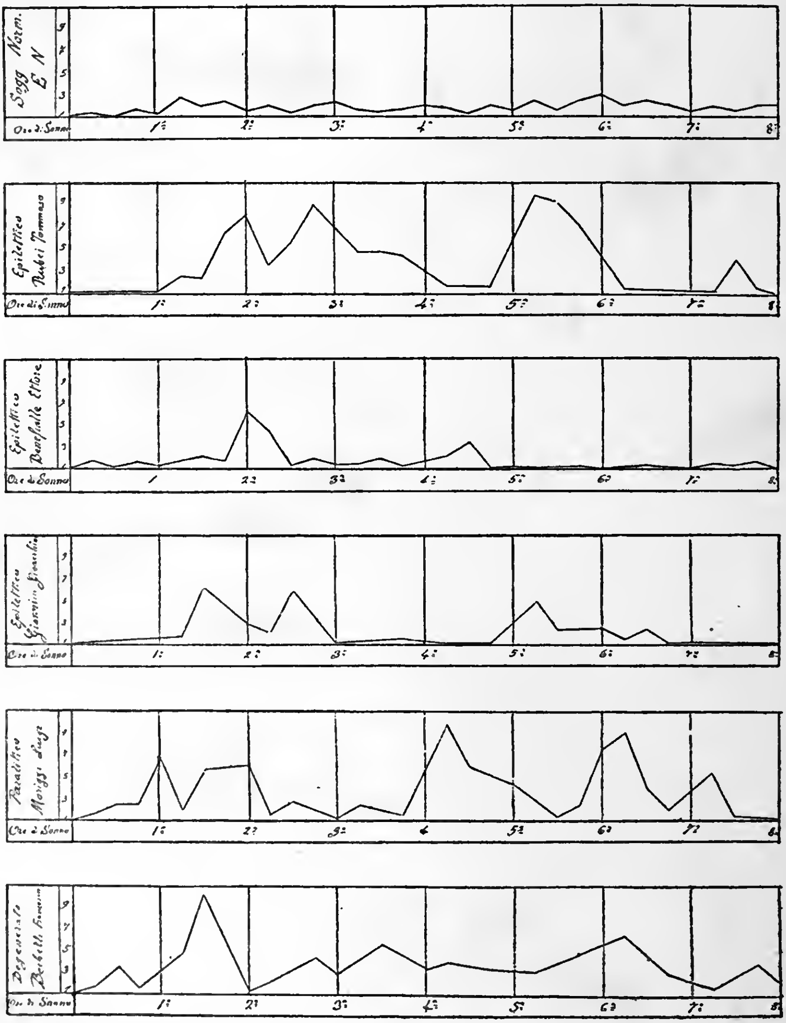
As to the maximum depth of sleep, in all the psychopaths, as in the normal cases, it falls in the first half of the second hour.

Examining the course of the curves for the psychopaths, we find that they differ among themselves much more than those of the normal subjects. In two only, *Benefialle* and *Bechelli*, does there occur a continuous lowering of height with continual oscillations, proceeding till the time of waking, while in the other three the curve presents peculiar characteristics. In the two epileptics *Rubei* and *Giannini* the oscillations are much fewer, but while in the former the depth is very great throughout, in the latter it remains at a much lower level. The curve of the epileptic *Benefialle* is the one which, as regards type, accords most with the normal, becoming continually lower, with oscillations, as the time of waking approaches; the curve of *Bechelli* comes next. In the paralytic *Moriggi* we find a type of curve which is more in accord with such normal curves as show a marked rise in the second half of the period. As to this rise, which we have termed *secondary*, the fact is to be

[1] Cf. De Sanctis: 'Die Träume,' etc.

noted that the two subjects in whom it appears most prominent are not dreamers at all, which does not bear out Lambranzi's hypothesis, at least for psychopaths; while, *per contra*, the two subjects who dream most frequently, *Rubei* and *Benefialle*, do not show it in any marked degree.

FIG. 11.



Relation between Subconscious Reactions and Waking-point in the Normal and Pathological Subjects.

TABLE VI.—RATIO BETWEEN SUBCONSCIOUS REACTION AND WAKING-POINT.

Hour of Sleep.	Normal.				Psychopathic.				
	E. N.	An. N.	G. N.	O. N.	Rubei.	Benefialle.	Giannini.	Moriggi.	Bechelli.
0.15	1.15	1.14	1.26	1.30	1.00	1.55	—	1.50	1.71
0.30	1.00	1.11	1.25	1.45	1.00	1.05	1.00	2.42	3.42
0.45	1.53	1.30	1.23	1.65	1.00	1.70	1.13	2.50	1.55
1	1.25	2.00	1.28	3.18	—	1.25	1.60	6.75	—
1.15	2.75	3.00	2.75	1.30	2.55	—	1.83	1.92	5.00
1.30	2.18	1.50	1.42	3.12	2.40	2.11	6.20	5.62	10.10
1.45	2.50	1.16	1.33	3.00	6.33	1.92	—	—	—
2	1.50	1.50	1.33	3.00	8.12	6.20	3.00	6.00	1.23
2.15	2.00	1.28	2.00	2.33	3.57	4.42	2.14	1.46	2.14
2.30	1.33	1.27	2.50	3.20	5.33	1.18	6.00	2.75	—
2.45	2.00	1.75	1.60	3.62	9.06	1.90	—	—	4.25
3	2.33	2.00	2.00	2.00	—	1.48	1.00	1.16	3.00
3.15	1.83	2.00	2.40	1.75	4.74	1.50	1.14	2.33	—
3.30	1.40	1.60	1.25	1.25	4.85	1.83	—	—	5.80
3.45	1.83	1.50	1.22	1.77	4.54	1.12	1.55	1.33	—
4	2.00	1.66	1.75	2.00	—	—	—	—	3.60
4.15	1.91	1.80	2.50	1.80	1.84	2.08	1.00	10.50	4.00
4.30	1.15	1.25	1.50	2.33	—	3.60	—	6.00	—
4.45	2.00	1.33	1.25	1.75	1.80	1.00	1.00	—	3.30
5	1.60	2.00	1.50	5.00	—	1.11	—	4.50	—
5.15	2.50	3.00	1.50	4.00	9.85	1.09	5.00	—	3.14
5.30	1.66	2.28	2.00	1.50	9.22	—	2.25	1.25	—
5.45	2.50	2.66	2.00	3.00	7.00	1.17	—	2.62	5.00
6	3.00	1.66	1.75	3.00	—	1.00	2.40	7.60	—
6.15	2.00	1.14	1.65	2.66	1.23	1.11	1.50	9.00	6.60
6.30	2.50	2.25	2.00	2.00	1.08	1.28	2.20	4.00	—
6.45	2.20	1.20	1.85	2.80	1.00	1.09	1.10	2.00	3.00
7	1.50	1.25	1.25	2.00	—	1.00	1.14	—	2.33
7.15	2.00	1.50	1.33	2.66	1.09	1.63	—	5.50	1.72
7.30	1.50	2.00	1.33	1.75	4.00	1.50	1.10	1.00	—
7.45	2.00	2.00	1.50	1.52	1.64	1.65	—	—	4.00
8	2.00	—	—	1.75	1.00	1.06	1.60	1.00	2.00

If, now, we turn our attention to the curves of subconscious reaction, we observe at once that they do not take a course constantly proportional to the curve of waking; now they approach, now they recede from the latter. Other observers have already noted that the reflexes have various modes of action during sleep; thus Marie de Manacéïne maintains that in sleep the reflexes are more vigorous and quicker to act than in waking, being no longer 'réprimés et maîtrisés par la vie cérébrale consciente'; this view is contradicted by Tarchanoff, who, after cutting the spinal cord of young dogs above the lumbar region, found that the reflexes in the hind legs, which then depended on the cord alone, did not alter notably from the waking state, while

those of the front legs, which were still under the control of the brain, underwent very marked diminution. This hint will suffice to show how important is the analysis of the curve of subconscious reaction in all our subjects.

How much higher a ratio between subconscious reaction and waking-point is maintained in the psychopaths than in the normal subjects, is shown numerically in Table VI. and graphically in the curves. (Compare Table VI. and Fig. 11.)

It is readily seen that the disproportion between the curve of the waking-point and that of subconscious reaction does not become very accentuated in the normal subjects. During the first hour of sleep in all the subjects, both normal and psychopathic, the ratio between subconscious reaction and awaking is very small and nearly uniform for the entire hour, which would go to show that the reflex activity remains at a very high threshold; while in the next hour the ratio in every case takes a very much higher value, and it is precisely in this hour that the depth of sleep reaches its maximum. In the succeeding hours we find that the course of the curve of subconscious reaction is less regular among the psychopaths than among the normal subjects. In the epileptic *Rubei*, in whom the depth of sleep is greatest, we also find the greatest difference between the two curves; it is worthy of mention that the ratio remains at a very high level for two thirds of the duration of sleep. In *Benefialle*, on the contrary, the differences between the two curves are, in general, not at all marked. They are rather more marked in *Bechelli*. In *Giannini* the greatest difference occurs between the first hour and a half and the second hour and a half; in *Moriggi* a maximum difference occurs not only between the first half hour and second hour of sleep, but also in connection with the secondary rise of the waking curve (sixth hour). As regards dreams, it is important to note that they occur in every period of sleep, even at the beginning, *i. e.*, when the depth is greatest; but they are more frequent and more vivid in the later hours of the night, particularly towards morning. As to the modifying influence of external stimuli on the course of dreams, we were several times able to note it. Thus the subject An. N. related that while she was dreaming of chatting with acquaint-

ances and was about to leave the house, of a sudden she was seized with a violent headache, so that she was compelled to turn back. (Precisely at that moment the experiment was being performed, as usual, with pressure on the forehead.) Another time the subject E. N. told of feeling as if she had just been bitten by a large spider. Similarly, O. N. dreamed several times of being engaged in a fight with other persons and of receiving a blow on the head with a stick or knife; etc.

The dream activity of the psychopaths, on the other hand, is slight. It is *nil* in the paralytic *Moriggi* and very small in the degenerate *Bechelli* and the epileptic *Giannini;* in the other two it is somewhat greater.

Further, in the psychopaths dreams occur more seldom in the first half of the sleep period and more frequently in the second; they are generally trivial in character, sometimes erotic, and the memory of them is for the most part very cursory. We were never able to observe any influence of the stimulation on the content of the dream.

It is worth while to note the tendency on the part of one subject to believe himself a greater dreamer than he really is. Are we dealing here, perhaps, with a fact of auto-suggestion ?

As regards the influence of the epileptic attacks on sleep and dreams we can say little, as these occurred very seldom during the period of experimentation. They seemed to exert no noteworthy influence in the case of the psychopaths; in *Rubei* they appeared to produce a diminution in the usual depth, and in *Benefialle* a slight increase. The recollection of dreams is less on nights in which attacks occur.

But our cases are too few to justify our laying stress on general conclusions. Moreover, inspection of the tracings will speak more plainly than any comment which we can make.

A word, in closing, as to the method. It seems to us that the depth of sleep may be satisfactorily measured by the method of tactile-pressure excitations. However, in future researches it will be necessary to employ an instrument with a spring having a greater length of compressibility and bearing, in consequence, a finer numerical gradation. To obtain this it is of course necessary to make certain changes in the ordinary

graduated esthesiometers and algesimeters. One of us is at present engaged in this task, namely, that of constructing a rational and practical *Hypnometer*.

ROME, August, 1901.

NOTE.—An abstract of this article was read by Professor Sante De Sanctis at the Fifth International Congress of Physiology, held at Turin, September 17 to 23, 1901.

DISCUSSION AND REPORTS.

POST-HYPNOTIC SUGGESTION AND DETERMINISM.

In his essay on 'The Dilemma of Determinism' ('Will to Believe,' etc., p. 145), Professor James says: "A common opinion prevails that the juice has ages ago been pressed out of the free-will controversy, and that no new champion can do more than warm up stale arguments which everyone has heard. This is a radical mistake." In agreement with this view the writer purposes to bring up a new line of argument. In the discussion of this question it has been generally agreed that we have nothing in the form of objective evidence to assist us, that, in the very nature of things, such evidence can only be subjective. Thus James, in the essay quoted above (p. 150): "Now, evidence of an external kind to decide between determinism and indeterminism is, as I intimated a while back, strictly impossible to find." So Hyslop ('Elements of Ethics,' p. 217): "The real weakness of the appeal to consciousness is that it can never have more than a subjective or individual value. It could not prove anything except for the individual who has it, and others might not possess any such power," and Simmel ('Einleitung indie Moralwissenschaft,' Band II., S. 306): "Wer es ausspricht (das Urteil, 'unser Geist ist unfrei') muss sich zunächst doch die Anwendung davon auf sich selbst gefallen lassen, und damit zugleich auf die Möglichkeit eines objektiven Nachweises seiner Wahrheit verzichten."

Now may it not be possible that we really *can* find something in the nature of objective evidence to throw new light on this question? If we give a young man, who is sleeping soundly, a slap in the face and see him awake in an instant, are we justified in believing that we know the cause of his waking? If instead, we tickle him until he awakes, have we anything in the nature of objective evidence as to the causes of his waking? Or must we accept his view of it, namely, that he 'simply awoke the same as at any other time?' Now let us take a case of post-hypnotic suggestion. We give the suggestion, and at the time set for its execution, the young man carries it out. Experimenters agree that we can foretell the results, and that the action of the young man has for its determining cause the suggestion given

him. Now is that opinion based on objective evidence or not? But the objection is made that such phenomena should not be considered at all in discussing this problem, that there is a vital difference between the process determining action in cases of post-hypnotic suggestion and that which takes place in cases of 'ordinary' volitional action. It is part purpose of this paper to show that there is no reason for believing that such a difference exists.

Frequently the assertion is made that the subject will fall into a slight hypnosis at the time he executes the suggestion, or that he feels himself acting under compulsion, or that afterwards he forgets about his having done what he did in carrying out the suggestion, or that while he is executing this suggestion he is easily brought to execute other suggestions given then and there. So, for instance, Lipps ('Suggestion und Hypnose,' p. 516): "Auch hier erneuert sich die Hypnose an dem Punkte, oder in dem Bezirke, dem die suggerierte Vorstellung in der Hypnose angehörte." Now the reports of different experimenters do not agree on this point, and in the experience of the writer, the manner in which the suggestion is executed depends upon a variety of factors. We find, for instance, in some cases, that the subject performs the action suggested in a perfectly natural way, in no wise differing from the way in which he attends to anything in his daily routine of actions. Again, we find him hesitating, or at other times still, actually passing through a struggle before executing the suggestion, or even refusing to execute it. Now how account for all this? If the order is, for instance, for him to get up and lie down on a couch after reading the daily paper for a few minutes, he will very probably do this without any sign of wavering on his part, and without there being anything in his manner that would seem in any way strange to those that know him best. If instead, he is asked to walk over to another chair (after reading a few columns) and to sit astraddle of it, facing its back, it is likely that he will show some slight hesitation, or that he will, before carrying out the suggestion, look sheepishly about him to see whether he is being particularly watched. If again, the suggestion is for him to go and lie down on a bed in an adjacent room, it is probable that there will be decided hesitation on his part before executing it, or even that he will not do so at all (unless he be a good subject who has frequently been experimented with). If, to bring out another point, he should be asked to get the hatchet and to break up a valuable piece of furniture, it is very probable that there will be a flat refusal. If asked afterwards in regard to the act suggested, the answers in the different cases will likely be different.

In the first case it may be: "Why, I don't know, it just occurred to me to do so," or something to that effect. In the second case the answer is likely to come: "Why, I just took a notion to," or "I just wanted to" or something of that sort and the subject is apt to show some annoyance in some cases. In the third case, if the action suggested was performed, especially if after a struggle, the subject will frequently recognize the fact that he was acting under suggestion.

It would seem that the nature of the action suggested, the manner in which it is given, and the character of the subject (including bodily state, as fatigue, etc.) are the chief factors that go to determine the results of the experiment. As regards the first factor, it has been the experience of the writer that when the action suggested is such as would in ordinary life be done without much thought and which would be in line with actions performed daily by the subject, it is executed without any hesitation and without any indication of hypnosis on the part of the subject at the time of the execution, and that questioning as to the reason for his having done so or so will generally elicit a response like that above under the first case. He does not seem to care to find out the reason why, in fact frequently doesn't seem to think that there was any reason why. That is to say, this will be the result if the suggestion is not given too forcibly. If the operator makes the mistake of impressing the suggestion too firmly in the mind of the subject, it is likely that it will startle him, thus make him suspicious, and, in many cases, enable him to detect the source; and, in case he should execute it, give him the feeling that he is acting under compulsion. Or, again, when the action to be performed is one that the subject would ordinarily not think of doing, something contrary to a habit of long standing, he will look puzzled or bewildered, will try to put it out of his mind, and yet, after he has once executed the suggestion, it is likely that he will say that he 'wanted to do it,' if asked about it. Of course, the nature of the action suggested, and the manner in which it is given, will not alone determine the result. The intelligence of the subject, his disposition, etc., are important factors. For instance, there are some subjects who, after the action, will always say, with a good deal of emphasis, that they did it because they *wanted* to, even in such cases where they seemed to go through a struggle before doing it. An intelligent subject, especially if he know something about the psychology of the matter, will be very apt to have his suspicions about many ideas that prompt themselves shortly after he has been in the hypnosis (it needs no saying that he will therefore also make mistakes at times).

In a general way, the truth of the matter seems to be this: as regards ordinary, non-moral acts that might come under the head of habitual acts, the testimony of the subject is generally that he didn't know why he did so or so, or that he 'just took a notion to do so,' or that he 'felt like it'; as regards non-moral actions contrary to habitual actions, especially such as seem foolish or distinctly in conflict with the views of the subject, the testimony, in case of an execution of the suggestion, is generally that he 'wanted to do it'; but where something immoral, or, for that matter, anything that is decidedly against the views of the subject, is suggested, the result is apt to be a refusal, and here we have, on the part of the subject, the consciousness of an 'outside' force or power, which seems to be present in the first two cases only when the suggestion given appears in consciousness with an intensity entirely out of proportion to the relative importance of the action suggested, due to a mistake on the part of the operator in impressing the suggestion too forcibly. This, in the mind of the writer, is a fact the importance of which has been somewhat overlooked. If we awake the sleeper by means of a slap he will probably be able to tell what woke him; if by tickling, the chances are that he will say that he awoke 'the same as any other time' (meaning that the cause of his waking was the same). So with post-hypnotic suggestion; if it is properly given the desired result will be obtained; there will be no indication of hypnosis; the subject will not be aware of its origin, and will furthermore tell us that he 'did as he wanted to.' This has been brought out by many experimenters. So, for instance, Bernheim ('De la Suggestion,' etc., p. 46): "L'idée suggéré se présente dans son cerveau à son réveil: il a oublié son origine et croit à sa spontanéité." So also Jordan, Forel, Schmidkunz, Sidis.

The suggestion will then, if the experiments are properly conducted, be executed by the subject under the impression that he *wanted* to do what he does, and, also, that he *could have done otherwise.* On the other hand, we know that suggestion and deed are linked together by a chain of causation admitting of no alternative. It is similarly a familiar fact that the testimony of the subject under the influence of ordinary hypnotic suggestion is to the effect that he could do otherwise than he does. Here we have subjective testimony to the effect that he could do so, and objective proof that he can not. The objection is raised here that this proves nothing for 'ordinary' volitional acts, that he is here acting under compulsion. Now the significant fact is this: He, the subject himself, feels that he is *not* acting under compulsion, that he is acting out of his 'own free will'—meaning, namely,

that he could have done otherwise. The testimony of consciousness (of the subject) here is to the effect that the process in the two classes of cases is identical. In the one class we *know* that he is not free to 'choose,' and yet he thinks he is. So we do know that in this case subjective evidence is worth nothing, and hence, even if this does not prove that he is not free to choose in the other case, it at any rate proves that the subjective feeling that he can choose does not prove that he can. So far we know that *subjective evidence in the one case is false*, and, therefore, may be *justified in believing that it may be false in the other case.*

Now to a consideration of those cases where the subject feels that he is acting under compulsion. What they really show is not evidence in favor of the indeterminist, but rather in favor of the determinist. We know, for instance, that when, in giving a post-hypnotic suggestion, we impress it too forcibly on the mind of the subject, he will, at the time the suggested idea appears in consciousness, feel that there is an alien force trying to gain possession of him, and that if he executes it, he feels that he acted under compulsion. Here the subjective testimony is to the effect that the will was determined, that the subject could *not* have done otherwise, and here *subjective evidence is not only corroborated by objective evidence, but actually by objective proof.* How does it come, then, that in the one class of post-hypnotic experiments subjective evidence is incorrect, and in the other correct? In the first case the suggestion is weaker (or more familiar, etc.), in the second it is stronger (or more uncommon, etc.). That is, if the suggestion is given in such a way that it does not come up with startling force, or if it be one that is not directly contrary to his general inclinations, the subject is not able to see that his decision is 'univocally determined'; when, however, it comes up in a way entirely out of proportion to the end sought, or, contrary to his views or character, he sees that his decision is determined and that there is no ability to choose to do otherwise. In the first case the subject's introspection does not enable him to see the true state of affairs, because the motivating power of the suggestion is only average in power, or because it is common in its nature; in the second case it does enable him to see the connection between the motivating power of this idea and the volitional (or the non-volitional, compulsory) act following, and it is because this one idea comes up in so powerful and unusual a way. It is generally admitted that the testimony of consciousness is unreliable in this matter, and so there is too much of a tendency to look at it as though that meant equally much for both sides of the

argument. So Hyslop ('Elements of Ethics,' p. 214): "If it be illusory [consciousness], argument on either side of the question is perfectly futile, for I have nothing but the testimony of consciousness to the cogency of the argument for necessitarianism." Again (p. 217): "But I do not think that its testimony can be either proved or impeached. It is itself the last resort for such truths as we actually believe, and it proves too much to discredit it and then accept other beliefs which it attests." Now it does seem that, in the light of the above analysis, argument on the one side is not necessarily futile, for the same shows us why the testimony of consciousness in the one case is right, and why it is wrong in the other.

But there is another way in which to get at this question of freedom of choice, and many of the experiments of the writer were conducted with this special purpose in view. Here of course only a few such cases can be considered, but it is his intention to give the whole matter a more nearly thoroughgoing treatment at some later day. Because of the great number of courses of action between which choice can seemingly be made, it is desirable, if possible, to get a case where we can leave open only two such courses, and then see whether it is possible to choose *either* the one or the other. Take the following case in point: A young man is given the suggestion that the first time he meets a certain friend of his, he shall tell him that he is foolish, and, as a reason for saying so, shall tell him, either: (1) that he spends his income too wastefully, or (2) that he devotes too much of his time to a certain young lady. At the first meeting he does tell his friend, after some hesitation, just as was suggested to him, that his friend is foolish. When his friend asked him for the reason why he made such an assertion, he told him that he squandered his money. Now what light does this experiment throw on the question? In the first place, it certainly must be admitted that the suggestion given has determined: (1) his telling his friend that the latter is foolish, (2) his giving a reason for saying so, and (3) that he can choose only one of two given reasons. Thus far, then, we have volitional action under the determination of post-hypnotic suggestion. But when he chooses one of these two reasons we must say that this is 'ordinary' volitional action. Of course everyone is ready to admit that, when he told his friend that he was foolish, it was impossible for him to do otherwise. Now the question is this: When he gave the first of the two reasons suggested, could he have urged the other reason? When asked afterwards why he had thus spoken to his friend he said: "Well, it occurred to me that he is foolish, and so I told him so." "But you know

that he isn't in the habit of spending more money than most of us." "Yes, I know; but I didn't want to offend him by telling my real reason." On being asked for that, he said that he thought that his friend was paying too much attention to a certain young woman. He 'chose' the first reason; could he, constituted as he was, desirous of not offending his friend, and knowing that it would offend him to give the real reason, could he have urged the other reason—could he have 'chosen' the other reason? To answer affirmatively would be to express the most evident contradiction. But before proceeding with this thought, it is instructive to note that the subject called the whole action his own, that there was no hitch or halt in the process when 'ordinary' volitional action followed action determined by suggestion—to all appearances *the process was the same in both cases.*

This particular case shows clearly that only the one reason could be 'chosen' and that under the circumstances existing at the time, 'choice' of the other was entirely out of the question. But it may be objected that in most of the 'ordinary' volitional acts there may be more than two alternatives. Very well. The suggestion here might have been given, as it was in other experiments, simply to the effect that *some* reason be given, without giving a list of those from which a selection was to be made. What ground would there be, then, for believing that whatever reason he gave, was *not* determined (by his character—his self)? Or to take another case: The suggestion is given to call A either a 'fool' or a 'coward.' He calls A a 'fool.' Even if we couldn't tell beforehand what he would call him, there is no reason for believing that his 'choice' was not determined. The fact of the matter is that if we had known the subject well enough, we could have predicted just what he would 'choose' to call A. And so with 'ordinary' actions. Place a man we know very well in a certain position and we can foretell accurately which one of two seemingly possible ways of action he will 'choose,' whereas, with regard to a man we do not know so well, there is only the element of probability in our prediction; and again, with regard to an absolute stranger, of whom we know practically nothing, and who is placed in such a position, we could only guess as to the course he will probably 'choose.' (This, in the light of what has been said, really amounts to a proof by concomitant variations, as was suggested to the writer by Professor Harlow Gale of the University of Minnesota. But it is not the province of this paper to take up this line of argument.)

Simply because we can not in 'ordinary' volitional actions isolate our antecedents and show the causal connection between them and the

volitional acts as consequents, there is no logical reason for disputing such connection. Where the young man above mentioned thought, that under the circumstances then existing, he could have urged the other reason just as well as the one he did give, we of course know that that was out of the question. And so in all cases, just as Schuppe says ('Ethik und Rechtsphilosophie,' S. 96): "Im konkreten Geschehenen gibt es selbstverständlich nur Notwendigkeit, ein 'Gekonnthaben' kann also immer nur den Sinn haben, dass etwas geschehen wäre wenn nur noch die und die Bedingungen hinzugekommen wären." It is not often that consciousness gives us the testimony in regard to 'ordinary' volitional acts, that they were determined. As Fowler says ('The Principles of Morals,' Part II., p. 330): "But it may be replied * * * that the reason is because I am not sufficiently acquainted with all the springs of action and their relative force, and that, when a man comes to reflect on the circumstances of his conduct, he often recognizes his past action as the necessary result of the various forces, internal and external, operating on him at the time." It is instructive to note that when we do so recognize an action of ours to be determined, it is not as a general thing one of the kind we call habitual, but rather one of an unusual nature, most likely one which came about after a severe inner struggle. After what was said in regard to the class of post-hypnotic actions where consciousness gives the testimony that the action was determined we can see why it is that consciousness is able to give that testimony here, and this amounts to *more evidence that the volitional process is the same in both 'ordinary' volitional acts, and acts determined by suggestion.*

One other objection must be considered here. It may be urged that in the case of the young man above mentioned, the suggestion was executed because it was not contrary to his nature (character). Very true. If it had been, he would, in all probability, not have executed it; yet, if in spite of this fact he had executed it, he would probably have said that he had acted under compulsion, as another man did who had been brought to slap a friend of his under the influence of post-hypnotic suggestion. It is this that Schmidkunz refers to ('Psychologie der Suggestion,' S. 276): "Wie die meisten, namentlich aber die misslungenen hypnotischen Suggestionen gerade für die Macht der zu überwindenden Widerstände zeugen, so zeugen die hypnotischen Suggestionen, insofern sie gegen die Selbständigkeit des Subjekts gerichtet sind, auch im siegreichsten Falle von der Existenz des Gegners." The answer to this and similar objections is simply this: The objection is not valid in so far as it is meant to show that

there is self on the one hand, and non-self on the other, either trying to overcome it or actually doing so, and that if the action is performed, it was performed by what was not self. The self at any moment of action is the previous self plus the last impression made upon it—plus the last influence motivating towards action, and the case where an action (of the above class) is performed under the feeling that it was compulsory is no exception. The action of the self may be likened to the action taken by a parliamentary body. Whatever action is taken, whatever the constituency of such body, the action taken is that of the self of that body, whether it took place after a change, either sudden or gradual, in the membership of such body or not. Similarly, in hypnotic experiments, we can change the membership, so to speak, of the self. We simply reduce or modify, at will, the real make-up, and through that, its expression, by means of suggestion. This may be done by inhibiting certain parts of this make-up, and leaving others free to gain their expression, as, for instance, when we suggest to the subject that he may be able to do certain things, but not others. Or, as in post-hypnotic suggestion, we can bring into the self a new factor, which, becoming a part of that self, helps to determine the expression of that self. Where this new factor is not of such a nature as to seek expression in a manner entirely strange to the self as constituted before the ingress of this new factor, it finds its expression, and the testimony of consciousness is to the effect that it was the act of the self. Where this suggestion *is* contrary to the character of what subjective consciousness has recognized as self, the testimony of consciousness will be to the effect that this was non-self, that the self was overpowered, or, in case of refusal to execute it, that there was non-self that tried to overpower self. A better psychological analysis shows how false this view is. Dewey ('Study of Ethics,' p. 129): "There is no factor foreign or alien to the agent's self; it is himself through and through." So also Stephen ('Science of Ethics,' p. 287): "When we say that his conduct is caused by one of those instincts, we do not mean that there is a man *plus* the instinct, but that the whole man, regarded as a unit, including this instinct, acts in a certain way in which a man (if such a man be possible) without that instinct would not act." That volition is the expression of the self, that it is determined by the character of the agent, is now generally admitted. "Which motive is chosen is perfectly fixed and dependent upon the character which cannot choose otherwise than it does." (Alexander, 'Moral Order and Progress,' p. 339.) "* * * but what it means is not that I might arbitrarily, or with no different

self have done otherwise." (Dewey, 'Study of Ethics,' p. 132.) "Zu sich selbst sagen: ich hätte anders handeln können, ist thöricht und sicher falsch." (Riehl, 'Der philosophische Kriticismus,' Band II., II. Teil, S. 271.) "As is its nature (that of a being), so it wills; as it wills, so it acts." (Carus, 'Monist,' Vol. III., p. 87.) "Ein Mensch handelt in einem jeden Augenblick seiner derzeitigen Beschaffenheit gemäss." (Gizyčki, 'Moralphilosophie,' S. 219.)

After having attempted to show that the volitional act is determined, with the self as the main determining factor, the next step would be to take up the question as to whether that self itself is determined, but that is not the purpose of this paper. And so, also, the question of responsibility cannot be taken up here any more than to quote from several writers. Stephen ('Science of Ethics,' p. 285) says: "I do not diminish a man's responsibility when I 'cause' him to act, but only when I cause him to 'act involuntarily.'" And Dorner ('Das menschliche Handeln,' S. 129): "Man behauptet vielfach, dass alle Verantwortlichkeit mit Leugnung der Wahlfreiheit aufhöre. Allein das Ich, welches thätig ist, rechnet sich die Handlung einfach deshalb zu, weil es bewusst thätig ist, also sich als handelnd weiss. Ob diese Handlung aus Wahlfreiheit geschehen ist, kommt hierbei nicht in Betracht. Das Ich ist thätig, und weiss sich als thätig: die Verantwortlichkeit ergibt sich aus dem Bewusstsein der Aufgabe, nicht der Wahlfreiheit. Weil ich etwas vollführen soll, bin ich verantwortlich dafür. Freilich hört meine Verantwortlichkeit auf, wenn mich äussere Umstände, die ich nicht beseitigen kann, an der Erfüllung meiner Aufgabe hindern."

In conclusion, where most of the representatives of modern philosophy are agreed that the volitional act is determined, it has been the attempt of the writer to bring forth some evidence that will strengthen the position of the determinist still more. He has thus attempted to show that there seems to be no valid reason for believing that there is any difference between the volitional process in 'ordinary' volitional acts, and acts performed under post-hypnotic suggestion; that to the subjective consciousness there is ordinarily no such difference; that when there is such difference attested to by consciousness it can satisfactorily be accounted for by the explanation as to when the testimony of consciousness is apt to be right, and when wrong; and that even though all that has been said does not prove the absence of such difference, it must be assumed to do so in the absence of disproof.

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PSYCHOLOGICAL LITERATURE.

The Play of Man. KARL GROOS, Professor of Philosophy in the University of Basel. Translated with the author's coöperation by ELIZABETH L. BALDWIN, with a preface by J. MARK BALDWIN. D. Appleton & Co. 1901. Pp. v + 406.

This sequel to the author's 'Play of Animals,' which was also translated by Miss Baldwin, is even richer in fact, discussion, and theory than its predecessor. The same practice theory of play is adopted, and the general plan of treatment is similar. But corrections and supplementations of the earlier pioneer work, that have occurred to the author on further reflection, or that have been suggested by critics, appear at many points. Notable among these are a more careful explanation, in the introduction, of the 'instincts' or 'impulses' upon which play is based, and a more mature theory of imitation. The theoretical discussions also are considerably developed.

In the introduction the author tells us that "among higher animals certain instincts are present which produce activity that is without serious intent, and so give rise to the various phenomena which we include in the word 'play,'" and that the same is true of man. But, in order for this statement to hold, the word instinct must be used in an extended meaning. Usually it means 'an inherited association between stimuli and particular bodily reactions.' But the imitative impulse is the basis of many plays, and yet it does not lead to particular reactions, and does lead to reactions that are intellectual rather than bodily. Moreover many actions, both physical and intellectual, seem to be due to a certain primal 'need for activity,' not for particular activity, but for activity in general. To use the word instinct for tendencies to activity so heterogeneous and unspecified, would be, in view of the technical meaning of the term, to run the risk of endless confusion. On the whole then Professor Groos is inclined to designate the basis of play as 'natural or inherited impulse,' though he is aware that impulse has an exclusively physical connotation in its more exact usage, and does not deny himself the use of instinct in contexts where no confusion would arise.

In the introduction the author also lays down the two chief criteria of play, the biological criterion, that 'it shall deal not with the serious exercise of the special instinct, but with practice preparatory to it,'

and the psychological criterion, that the act shall be performed 'solely because of the pleasure it affords.'

The main body of the book, covering some 350 pages, is primarily analytical and descriptive, while the conclusion of 46 pages deals with theories of play, physiological, biological, psychological, etc. Omitting these theories for the present, a hasty glance will give some idea of the topics discussed in the analytic portion.

Play being based upon 'inherited impulse,' a classification of impulses is adopted as a basis for the classification of plays, not because the resulting arrangement is unexceptionable, but because it 'may serve to open at once to the reader the inmost core of the subject,' and at the same time be sufficiently comprehensive of the main types of play. Plays are accordingly divided into two main groups, those that train the individual to manage his own psycho-physical organism, and those by means of which he gains control over actions of his 'directly concerned with his relations to others.' For plays of the first type the somewhat unfortunate and puzzling designation 'playful experimentation' (in the original, *spielende Experimenten*) is adopted. This does not mean that such plays are entered into for the conscious purpose of gaining knowledge or of discovering the best means to chosen ends, as Professor Groos's readers, scientists for the most part, might naturally suppose; for such activity would not be playful. Nor does the term connote the blind experimentation by trial and error, characteristic of the learning of animals; for that, too, is serious. Playful experimentation is merely an exercise of impulse that in fact leads to the acquisition of skill, though it is guiltless of any such intent, or indeed of any ulterior intent. One does not see why the editor's happy suggestion of 'autonomic' impulse was not here substituted, especially as 'socionomic' impulses, also his suggestion, is used in the translation to designate the second order of impulses above referred to.

The playful exercise of autonomic impulse, to use that convenient term, falls into three main groups. Sensations of the various senses, of all kinds and in all combinations, may be sought for the enjoyment they give; noteworthy here being delight in the production of sounds, the playful basis of speech and all it implies, and delight in the perception of form, the basis of drawing, etc. Many plays consist in the exercises of the motor apparatus, whether the bodily organs be moved, or foreign bodies be moved through their agency; here belong destructive and constructive plays, playful feats of endurance, throwing and catching plays, etc. Finally, the higher psychic powers may

be playfully exercised; as in memory, imagination, and reasoning plays; or even pain, in the most general sense of the word, may be playfully sought, as in the worrying of a sensitive tooth; and many games essentially consist in the exercise of will power, as where winking or laughter has to be suppressed.

Plays of the second or socionomic order, on the other hand, fall into four groups, fighting, love, imitation, and social plays. Among the first are included playful rivalry, teasing, hunting, witnessing fights, and the tragic. Under the second head we find discussions of courtship and sex in the comic. The third group contains discussions of dramatic, plastic, and inner imitation, the last being an extremely interesting account of Clifford's ejecting process in its playful exercise. Here imitation itself is also discussed at some length. Social play is considered rather from the standpoint of theory, and this section serves as an introduction to the general theoretic discussion that immediately follows. The classification serves well the author's dual purpose, being comprehensive and giving the reader an insight into the heart of the subject.

Under each of the subheads above suggested examples of the plays both of children and of adults are briefly described and carefully analyzed. It would be interesting to follow the author into detail, but that is of course impossible here. Moreover Professor Groos's book is much more than a mere descriptive catalogue of human plays. To be sure, in the interest of thoroughness, it is compelled to be that in part, and thus continuous reading becomes just a little tedious over some stretches. But, on the other hand, the value of the volume as a book of reference is greatly increased by the author's thorough treatment of his extensive field. And in addition much that is interesting and stimulating appears at frequent intervals. For under Professor Groos's scholarly and fruitful treatment discussions of various types of play are made to throw light on many obscure problems, in psychology, biology, sociology, and æsthetics. Indeed few pages intervene, for the most part, between illuminating discussions of important problems. A few examples will give some idea of this aspect of the book.

After discussing sound-play among children and adults, and pointing out that sound as such, but especially exciting sound, is enjoyed and even craved, the author discusses the question, 'Whence is derived the strong emotional effect (1) of rhythm and (2) of melody?' Darwin's theory that this pleasure arises as an effect of sexual selection affords, to Professor Groos's mind, a partial but not a radical explanation. Song plays a part in the courtship of birds, but their kinship to us is

remote. Mammals generally rely little on courtship, monkeys hardly at all, and most of the latter, moreover, make no use of sounds in that connection. Besides, among primitive races music does not seem to be very closely related to sexuality.

The relation of rhythm to the emotions is intrinsic rather than adventitious, thinks Professor Groos. The rhythm of sound is in deep accord with the rhythm of our organic processes, with heartbeat, breathing, the step, etc., and with the pulse of attention itself. And, probably because of this accord, rhythm easily induces a state of semi-trance which, as Nietzsche, Souriau and others have shown, is the precondition of æsthetic delight. But the trance state is merely the precondition; the suggestibility incident thereto is the portal through which our æsthetic pleasures enter. Rhythm entrances us; the other elements of music suggest to us pleasant dreams.

And among these other elements the most potent is melody, whose power can best be explained by saying 'that it makes the impression of a dancing voice.' For, in the first place, melody is the quintessence of movement, movement disembodied, rid of the grossness of substantiality, present to consciousness in its naked purity, and stirring us as living movement alone can. And, in the second place, melody, in its enchantment of onflowing tones, is a voice, speaking a language of its own, addressed to the emotions, and capable of arousing them, from the tenderest to the boldest and most massive, in all combinations of accord and discord.

In short, sound, and especially rhythm and melody, being intrinsically delightful, are sought and played with by child and adult, and thus out of productive sound-play music has developed. Courtship has merely influenced the growth of music, and that chiefly in its later stages. Professor Groos returns often to Darwin's theory of the relation of art to sexual selection, and especially in discussing love play in art he considers it together with Grant Allen's closely allied views and concludes that play is the principal source of art. His theory certainly explains well the essential sportiveness of art, and throws light on a certain irresponsible sportiveness characteristic of artists, and besides is fortified by facts, discussions, and citations of authorities, that cannot be repeated here.

Though familiar to many from its mention in 'The Play of Animals' and elsewhere, the author's theory of the origination of attention in the animal instinct of lying in wait deserves mention. Warned of the proximity of prey or enemy, the animal restrains his whole body to stillness to avoid all betraying sounds, holds his muscles tense, and

braces his entire organism for appropriate action at the proper instant. This preparedness and expectancy becomes, when constantly renewed, the concentration on an object that develops into theoretic attention.

In discussing dramatic imitation Professor Groos replies courteously to Hall and Allin's remark, in their article on tickling, etc., that his theory of practice and preparation is 'obviously wrong' in the case of the playful imitation of animals by children. Imitation in general, he insists, is of great biological importance as practice, and its extension to animal actions is not remarkable. Besides, a thorough knowledge of animals is essential to primitive man, and playful imitation yields knowledge the most thorough and intimate. As against his critics' theory, advanced in partial explanation, that the animal-like behavior of children is a recapitulatory development of animal instincts that, growing to a maximum, will be controlled and subdued by higher powers duly unfolded, Professor Groos objects that the child imitates animals only with effort, 'and this at a time when it has already progressed very far in the acquirement of human capabilities' (p. 303). Professor Groos's defence is strong and his objections weighty. But, while Hall and Allin's theory cannot explain the wide-spread imitation of animals, for the reasons mentioned, because the imitation is often conscious, and because many of the animal species imitated do not fall within man's ancestry, yet it explains well much animal-like behavior among children, especially in quasi-pathological cases.

Of the many other important discussions in the section dealing with imitative plays we have space to mention only three: the account of the relation of imitation to instinct; the brief discussion, satisfying as far as it goes, of the origin of language; and the description of inner imitation. To prevent misunderstanding it should be said that imitation in the author's usage is not synonymous with Tarde's broad meaning, nor with Baldwin's 'circular reaction,' but rather with Lloyd Morgan's 'repetition of the acts of one individual by another.' This usage is adopted as a matter of convenience, not in disapproval of the broader connotations.

Premising that in man imitation is not so much a temporary substitute for instinct, according to the thought developed in Baldwin's theory of organic selection, as a type of action supplementary of the instinctive that tends to 'relegate instinct to the category of things rudimentary,' Professor Groos proceeds to raise the question whether we might not be justified in calling the imitative impulse itself an instinct. The difficulty in the way of an affirmative answer is that

'in imitation we have a thousand varying reactions,' in place of one clearly defined reaction, and it is therefore necessary to proceed with caution.

But in imitation two points are reasonably clear, first the existence of certain more or less generalized types of reaction as hereditary possessions, and secondly, in conscious, or what is sometimes called voluntary action, the tendency of the idea of an action to produce the action itself. Song birds may learn to sing, little girls to 'nurse,' boys to fight, even when no models offer themselves to suggest ideas of these actions; there are general tendencies to action in these directions, and in 'playful experimentation' somewhat satisfactory performances may be learned. But when good models are presented, both the performance of the action and its relatively superior quality are insured. Imitation seems therefore to be based on more or less rudimentary or generalized instincts, that impel to types of reaction rather than to specific reactions, and to find its supplementary factor in dynamogenic ideas suggested by the observed conduct of others. The model without the instinctive basis has not sufficient force—boys seldom 'nurse'; the rudimentary instinct without a model may develop into action, but the action will be imperfect, and, besides, will not be imitative. Thus, in the higher animals and man, imitation, or more generally tradition and social heredity, put the finishing touches on generalized instincts, and make possible a nicety and flexibility of adjustment unattainable where instincts are rigid. Professor Groos also points out that curiosity, 'How does he do it?' the fighting instinct, 'I can, too,' and the pleasures of recognition and illusion, make powerfully for imitation. And other varying groups of instinctive aids are present in different cases of imitation.

So far imitation is closely connected with instinct, but is not itself an instinct. But Professor Groos notes the keenness of the pleasure imitation gives, the strenuousness of the impulse to imitate, and the seriousness of the disappointment in case of failure, and inclines cautiously to the opinion that these may be 'direct results of selection and the developmental factors connected with it.' If so, imitation is a 'phenomenon at least similar to instinct.' However, conclusions here are doubtful, and the author is too cautious to commit himself.

The author gives his theory of the kinds of sound out of which language develops in a paragraph. It is not out of sounds imitated from those heard in the environment, nor out of interjectional sounds, nor out of both that the whole of language develops. For many sounds, *e. g.*, mamma, papa, adda, arise from neither source, being rather

the results of 'playful experimentation.' Language grows up then as a result of the imitation of all three, of natural, of interjectional, and of experimental sounds. It is to be regretted that Professor Groos has not addressed himself to explaining how sounds that in fact convey meaning come to be uttered in order to convey meaning. His familiarity with the facts in the field of animal psychology would give great weight to his conclusions.

Much has been made of late, especially in Germany, of inward sympathy as a most important factor in æsthetic enjoyment; some, for instance Lipps, going so far as to hold that æsthetic enjoyment *is* agreeable inward sympathy. Professor Groos carefully analyzes the complex state of inward sympathy or inner imitation, and describes at some length its relation to æsthetic enjoyment. In inner imitation we conceive of the inner experience of others, even attributing psychic states to lifeless objects; we participate in the movements of persons and things and conceive of tensions and stresses in bodies at rest; and we transfer in thought our resulting emotions to whatever arouses them, speaking of 'the solemnity of the sublime, the gaity of beauty, etc.' As Lipps says of the Doric column, "The vigorous curves and spring of such a pillar afford me joy by reminding me of those qualities in myself and of the pleasure I derive from seeing them in another. I sympathize with the column's manner of holding itself and attribute to it qualities of life because I recognize in it proportions and other relations agreeable to me." All this Professor Groos admits, for he too maintains that inner imitation forms a large part of æsthetic enjoyment. But he supplements Dr. Lipps's statement by important additions, the first three enriching the concept of inward sympathy with further specifications, the fourth setting forth that play is a neglected factor, which, joined to inner imitation, rounds out æsthetic enjoyment. First, the beautiful object does not, as a starting-point of association, merely call up in succession qualities, etc., formerly experienced in oneself. The situation is not one where states are aroused in succession by association, but one where all the elements are co-present and intimately fused into unity. Secondly, the sympathetic inner state movement and mood do not consist of mere ideas of past experiences. They are present live states; in admiring the Doric column one actually braces oneself as if for support, etc. But thirdly, the sympathetic movements, stresses and strains are symbolic, not overt acts, as Lee and Anstruther-Thomson apparently believe. And fourthly, inner sympathy may be aroused, as when thunder is heard as an angry voice, without æsthetic enjoyment. Only when the state

is pleasant, and is lingered over and *played* with is there æsthetic enjoyment.

The theoretic discussion that closes the book is in part, though by no means wholly, a summary of conclusions already reached, considered from six points of view, the physiological, the biological, the psychological, the æsthetic, the sociological, and the pedagogical. As much as is here possible has been said of the author's biological and æsthetic theories, and the length this review has already reached forbids any mention of his sociological and pedagogical views, the latter being besides least fully worked out. But a few words must be added on the physiological and psychological theories of play.

Play in childhood and youth finds its physiological explanation largely, according to Professor Groos, in the presence of surplus energy, as Spencer rightly holds; but the plays of adults, or at least many of them, must depend on the rival recreative theory. To be sure the two theories are often mutually assistant, weariness in one direction leading to the desire for recreation, while surplus energy in another direction calls for playful exercise. But it not infrequently happens that recreation is found in the exercise of functions whose energy is merely normal or even subnormal, and sometimes even in just a variation in the exercise of a tired function, as when an investigator turns from one aspect of his problem to another.

Moreover there is one familiar fact that neither theory successfully explains, the fact of the continuation of play to the point of exhaustion, and, if possible, beyond. This the author explains as a result, partly of Baldwin's circular reaction, and partly of the state of semi-trance induced by many plays. A critical comparison of this physiological theory with Marshall's pleasure-pain theory would be interesting at this point did space limitations permit.

The two psychological criteria of play that Professor Groos accepts are, 'its pleasurableness, and the actual severence from life's serious aims.' The consciousness that the play is a sham counterpart of a serious activity, which Wundt defends, the author rejects as a universal criterion. Objectively play is such a counterpart, but subjectively it is often too absorbing to be so interpreted. The little girl engrossed in her doll, the boy absorbed in his toy soldiers, have no such consciousness. Of course the severance of play from life's serious aims, or, a little more exactly, from any ulterior aim, is what differences it from work. And if it be objected that the distinction is often impossible of sharp application, immediate and ulterior aims differing in degree only, Professor Groos is ready with the answer that play and

work are not in fact sharply different, since, on the contrary, one often shades imperceptibly into the other.

But a more subtle and difficult psychological distinction is that between plays and pleasures, and this unfortunately the author has not attempted to draw. The closeness of the two concepts is evidenced by the striking likeness of Marshall's pleasure-pain theory and the author's physiological theory of play. Indeed it may be suggested that 'The Enjoyment of Man' would probably have been a more accurate title than 'The Play of Man.' For witnessing a theatrical performance, admiring a Doric column, and in general what the author calls receptive plays, are more naturally called enjoyments, or, to bring out the whole difficulty at once, pleasures, than plays. But while Professor Groos does not expressly face the difficulty, he offers material for its solution in many passages, and it may be for that reason that he does not feel the need for explicit discussion. He repeatedly remarks that the pleasure in being a cause, *i. e.*, in conscious production, is present in practically all plays, and in discussing receptive plays he emphasizes the fact that inner imitation, with its keen sense of activity, is an essential factor. Again, the remark frequently recurs, that the mere presence in consciousness of a pleasure does not make the state playful; it is essential that the pleasure should be actively sought and lingered over. And finally, play is always an impulse in actual exercise. In short, all plays are pleasures of activity, and, while it is not true conversely that all pleasant activities are plays, it is true that pleasant activities become plays when performed for their own sake. Evidently an understanding of play will throw much light on the difficult questions of the nature of pleasure, and of its place and function in life.

A good translation, which this one is, of so valuable a work into English is a subject for congratulation. But it is unfortunate that the proofreading was not more careful; see, for instance, the last paragraph ending on page 381. It is unfortunate too that a book so rich in fact and theory, whose arrangement frequently compels the same problems to be discussed from different points of view at different places—that what is in short essentially a book of reference, should not have a subject index; the index of authors is not an adequate substitute. Another addition that would greatly improve a second edition would be full footnote references by the translator or editor to authorities on play writing in English.

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Pascal. AD. HATZFELD. Les Grands Philosophes Series. Paris, Félix Alcan. 1901. Pp. 291.

M. Adolphe Hatzfeld, who died the year before this work on Pascal came from the press, was trained in that famous nursery of scholars the *École normale supérieure*, and later was professor in the Lycée of Louis-le-Grand. The volume is one of the series: *Les Grands Philosophes.* The chapters (Part Third) on the scientific writings of Pascal are written by Lieutenant Perrier, member of the French Geodetic Survey; the rest of the book is from the hand of M. Hatzfeld. The work is a comparatively full and skillful statement of the fundamental facts in Pascal's spiritual development, admirably reviewing his services to science, philosophy, and religion. There are few facts of an external kind to tell concerning Pascal's career. His life was little connected with affairs, and M. Hatzfeld makes this plain in the title of the biographical chapter, *biographie psychologique.* The outward life is noticed only in so far as it serves to explain the man's thoughts or beliefs. It was important for Blaise Pascal that when he was eight years old (b. 1623, his mother died 3 years later) his father, Étienne, resigned the Presidency of the Court of Aides, at Clermont, and moved to Paris, where he had superior opportunities to advance the education of his only son. The father was himself a considerable mathematician and well trained in science and literature. The great contemporary mathematicians Fermat, Mersennes, Roberval, and Le Pailleur were frequent visitors at his house, where Descartes also visited at different times. From the date of his resignation, his sole passion appears to have been the education of his son. Blaise early exhibited prodigious talent for mathematics, but was kept from their study and instead was carefully instructed in the teachings of Christianity, the precept being strengthened by the father's noble example. His education was carried on almost exclusively under his father's direction, who did not suspect that the extraordinary diligence of the precocious boy added to the gravity of his physical weakness. These facts of the father's guidance in mathematics and religion, the tense application of the nervous, delicate child, and his late renunciation of science for a cloister-life, were of primary significance for Pascal's career. His chief importance in the history of thought lies in the domains of mathematics and religion, and he appears to have abandoned the former for the latter, partly, at least, for reasons to be sought in his pathological condition. M. Hatzfeld, who writes entirely from the standpoint of the *vie de Pascal* written by Mme. Périer (*née* Gilberte Pascal) and Catholic orthodoxy, sees nothing neuropathic in

Pascal's condition. Even the famous *amulette* is regarded by M. Hatzfeld as only a triumphal cry of the faithful, a staccato note of spiritual joy! (p. 53). The exaggerated asceticism (*e. g.*, his wearing a belt of spikes), his morbid antipathy to marriage exhibited in his advice to his niece, the awful abyss that yawned before him from time to time and of whose unreality he found it hard to convince himself, his belief in the miracle of the Holy Thorn, his plunging one year into the gaities of social life (in 1652 he is said to have been enamoured of a lovely *femme savante*, then known as the Sappho of Auvergne) and another year withdrawing into the solitary life of an anchorite—these seem to M. Hatzfeld to be normal marks of an exalted religious devotee, while to most observers they no doubt suggest a neurotic taint in Pascal's genius. His last years, the years of the *Pensées*, are the legitimate product of his early education, an education devoutly religious on the one hand and scientific on the other—combined with a nervous, delicate organization.

Cousin, who through his rediscovery of the Pascal MSS. in the *Bibliothèque nationale* (then *royale*) and his famous report on the same to the Academy has done more than any one else in the century to revive interest in the great Port Royalist, considered Pascal the declared enemy of all philosophy, because he was a complete and avowed sceptic. He says Pascal took refuge in religion as our last resource in the impotency of reason. Against this charge of scepticism and the rejection of philosophy, M. Hatzfeld replies that Pascal does not reject philosophy and science, but merely delimits the domain of their puissance and certitude. This, to be sure, is a survival of the old standpoint of mediæval theology—the standpoint of a twofold truth. Pascal's mind was fashioned by three main influences. Augustine and the Middle Age philosophy, the scepticism of Montaigne, and the discipline of mathematics, which last furnished to the seventeenth century its ideals of philosophy. In the *Pensées* we find everywhere the spirit of Montaigne combined with the religious philosophy of Augustine, and the form of statement is fashioned after the models of geometry. Pascal was in the legitimate sense of the word a sceptic. He discovered in the nature of reason itself, as an inadequate instrument for the acquisition of the highest truth, the motive for the subordination of reason to faith. He made a metaphysic of scepticism subserve religion and, as a true Roman Catholic, regarded his philosophy as *ancilla theologiæ*. The contrarieties or antinomies in the reason itself, its feeble inadequacy on the one hand and its majesty on the other, are best illustrated by an extract from his conversation with

M. Saci in 1654 (preserved and reported by Fontaine) regarding Epictetus and Montaigne, the two authors whom Pascal most read. "I cannot dissemble that in reading Montaigne, and comparing him with Epictetus, I find in them the two greatest defenders of the most celebrated sects of the world, who profess to follow reason rather than revelation. We must follow one or the other. Either there is a God and a Sovereign Good, or this is uncertain, and all is uncertain—whether there is any true good or not. * * * The error in both is in not seeing that the present state of man differs from that in which he was created. The one, observing only the traces of his primitive grandeur and ignoring his corruption, has treated human nature as if it were whole, without any need of a Redeemer—this leads to the height of pride; the other, sensible of man's present misery and ignorant of his original dignity, treats human nature as necessarily weak and irreparable, and thus, in despair of attaining any true good, plunges it into a depth of baseness" (Tulloch's *Pascal*, p. 185). This contrariety in the nature of man is the central notion in Pascal's philosophy. Either of these two elements in man, when taken alone, gives a false and fragmentary presentation of his nature. The philosophy of Christianity explains the union of these two elements of human nature by the doctrine of the Fall and the doctrine of Redemption. The fact that Christianity in this way harmonizes with man's moral psychology is, in Pascal's view, the strongest proof of its truth (*Pensées*, Chap. IX.). The complete truth is discovered to us by revelation and is appropriated by an act of faith, but it is not discoverable by any process of reason. Pascal is, therefore, an absolute Pyrrhonist in philosophy, but precisely because of this Pyrrhonism in philosophy he is a profound and ardent believer in a higher truth. He is at once sceptic and believer—a sceptic as to the competency of reason to discover adequate truth and a believer in the competency of the heart and faith, as instruments of revelation. He admits the sufficiency and authority of reason in the domain of human science and asserts its futility in all that transcends this domain. Scepticism in philosophy is for him the logical antecedent of belief in revealed knowledge. *La foi est dans le cœur* (*Pensées*, X., xi.) and is higher than reasoned truth in the sense of supplementing reason in a domain into which reason's processes cannot penetrate. Pascal and Descartes agree in saying '*la croyance a besoin de la volonté*' (p. 260), but in the philosophy of Descartes it is the reason that moves the will, while in Pascal it is the heart or feeling that determines the will to assent or dissent. Pascal transcends the Pyrrhonism of Mon-

taigne by admitting the adequacy of spiritual instinct as a source of ultimate knowledge. The contrarieties in the nature of man indicated in the conversation with M. Saci furnish the keynote of Pascal's thought, of his scepticism and of his faith.

It is Montaigne's world of 'abysmal dilemmas' that has impressed itself on Pascal. "There is nothing more extraordinary in the nature of man than the contrarieties which are discovered in it on almost every subject. * * * We have a powerlessness for determining truth, which no dogmatism can overcome; we have a vague notion of truth, which no Pyrrhonism can destroy. We wish for truth and find within only uncertainty. We seek for happiness and find only misery. We cannot but wish for truth and happiness; yet we are incapable of attaining either" (*Pensées*, V.). This condition of helplessness united with aspiration towards a higher state represents man's condition since the Fall. In this condition all good and all truth are due to divine inspiration and revelation. Pascal's creed is a composite of logic and feeling, whose product is rational scepticism and spiritual faith. His creed is the creed of the mystic; his belief is a thing of the heart. Intellectually he is a true disciple of his master Montaigne, whose motto suited no sceptic better than Pascal: *Que sais-je?* Pascal simply wills to believe and prepares his heart for the light of grace in the spirit not of Augustine, Calvin and Jansen, but in the spirit of the semi-Pelagians (pp. 109, 275). The contribution of Pascal to philosophy consists mainly in his animating the old apologies for the Christian religion with what Voltaire called an '*éloquence ardente et impérieuse*' (p. 271). Further, after the manner of a physicist, he seeks some law to explain observed facts in human nature, especially the above-mentioned contrarieties of *grandeur et bassesse*, and he finds that Christianity by its doctrine of the Fall furnishes the only intelligible explanation of these opposing phenomena and the doctrine of Redemption their only remedy. The significance of his philosophy, as above noted, is to be sought chiefly in the domain of religion.

As to the famous 'wager' argument for the existence of God, which most moralists handle with an uncanny feeling, it is not an invention of Pascal, but was commonly used by the Port Royalists and is found even in Plato (*Phaedo*, 107C), as M. Hatzfeld points out. Further, Massillon, La Bruyère, and Leibniz regard the argument as logically valid and morally sound. Voltaire, on the other hand, calls it "un peu indécent et puéril; cette idée de jeu, de perte et de gain, ne convient point à la gravité du sujet" (Vol. XXVI., p. 237). Belief in God on this ground is evidently divorced from the claims of

truth and is rested entirely on claims of interest. Again, stating the situation thus: God either is or is not. If you stake your belief in the existence of God and win, you gain infinity and lose nothing; if, on the contrary, you place your wager on the side of God's non-existence, you win nothing and lose infinity. The criticism of Voltaire appears to be just. Further, the claims of interest here may be valid grounds for conduct, but they cannot be converted into valid grounds for belief. Claims of expediency and interest may require us to look up and down a railroad track before crossing, but they constitute no ground for believing a train is coming—in fact we may believe it actually unlikely, on the calculation of probabilities, that a train is coming. It is, further, not necessary, as Pascal assumes it to be, either to assert or deny the existence of that which may be regarded as indemonstrable or doubtful (Voltaire, *ibid.*).

Pascal was an ardent religionist from the time of his so-called second conversion (the accident of 1654, at the bridge of Neuilly). In that year he completely retired from the world, profoundly impressed with the position of man suspended here 'between the too abysses of infinity and nothingness, and equally remote from both,' with the triviality of scientific research, and with the supreme necessity of spiritual light and grace. To this end he accepted the ceremonial means of the Church and subscribed to its dogmas. Logic and mathematics were of no avail; they only tie us to the small world of sense—a world that grew hateful in his eyes, as he withdrew more and more into 'the renunciant life.' He practised a rigid asceticism worthy of the most stringent anchorite; marriage, the pursuit of secular truth, even the most innocent diversions, became cursed with a carnal taint. Relief and happiness were to be sought for in the work of grace, the application of holy water, and the purchase of masses, of which Pascal says: "We must conform our minds by following those who have observed the saving rites of the Church, *en prenant de l'eau bénite, en faisant dire des messes*, etc.; *naturellement même cela vous fera croire et vous abêtira* (*Pensées*, ed. Havet, 5th ed., Vol. I., p. 152). On this Leslie Stephen comments: "Drug yourself with holy water or be a brute beast! We reply, as the old Duchess of Marlborough replied to her doctor's statement that she must be blistered or die: 'I won't be blistered and I won't die.' We won't be drugged and we won't be brute beasts." (*Studies of a Biographer*, Vol. II., p. 282.)

The original character of the posthumously published *Pensées*, which were meant to be a systematic apology for Christianity, but which

Pascal, owing to feeble health, left a mass of disordered jottings, is best exhibited in the critical edition of Michaut (Fribourg, Suisse, 1896). The most judicial reconstruction of this fragmentary mass has been furnished by M. Faugère (in the new edition only the *Lettres Provinciales* have so far appeared, 1886–95), and M. Havet (5th ed., 1897).

Pascal's most notable literary achievement was, no doubt, the *Lettres à un Provincial.* In this series of 18 letters (from January, 1656, to March, 1657, put on the *Index expurgatorius* Sept. 6, 1657) in defence of M. Arnauld against the Sorbonne and the Jesuits, Pascal proved himself not only the greatest controversialist of the 17th century, but incidentally he became thereby the creator of modern French prose. Of these letters to an imaginary friend Voltaire says: "The best comedies of Molière have not more wit than the earlier letters, nor has Bossuet anything more sublime than the later ones (*Siècle de Louis XIV.*, Ch. xxxvii.). The letters were, in their apologetic function, directed to the defence of M. Arnauld and the doctrines of Jansenism; in their offensive character, they were aimed at the doctrine of probabilism in the ethics of the Jesuits. The letters worked more injury to that order than any attack made against it from its foundation by Loyola. The letters were read by everybody and went through numerous editions even in the life of Pascal. They are full of eloquence, of convincing fervor, of stinging satire galvanic in its effect on Pascal's contemporaries, and withal they are a perfect example of literary art. They filled the world with the fame of Port Royal and its cause, and they stimulated into being a great mass of replies, frail, impotent, lasting for a day. M. Hatzfeld points out the fact that Pascal was opposed to the real Jansen, the Dutch Calvin of the *Augustinus;* he is indeed the defender of certain Jansenists but not of Jansenism (p. 206). On the contrary, he was in his position regarding the subject of grace and free will (the Jansenists being determinists) a semi-Pelagian and no true disciple of Jansen. The feud between Jesuit and Jansen, immortalized by Pascal, was a feud in the main touching the obsolescent questions of sufficient, proximate, and efficacious grace and the use of casuistry. M. Hatzfeld reviews the controversy in its essential historical and logical features, a somewhat unsympathetic review from the Pascalian standpoint. Although M. Hatzfeld characterizes the style of Pascal's letters as filled '*avec une ironie charmante*,' yet the argumentation, he says, is unsound. "*Pascal retombe dans le même sophisme, lorsqu'il invoque pour égayer les lecteurs*" (p. 204). There is not space to say more here

about M. Hatzfeld's account of this great polemic, a thing now of interest chiefly to the historian of theology.

M. Perrier in his chapters on the scientific work of Pascal has furnished us with a history of a much neglected side of Pascal's life. His genius was fundamentally scientific, and had it not been for his associations with the men of Port Royal his achievements in this field would doubtless have entitled him to rank amongst the foremost discoverers in mathematics and the philosophy of nature. M. Perrier concedes that Pascal is in no sense to be called the creator of modern experimental method (p. 141), although the famous experiment of Puy de Dôme (1648) was perhaps the most notable of the early experiments in physics and effectually relegated to the limbo of myth nature's *horror vacui*. His unassisted solution of the twenty-third proposition of Book I. of Euclid's Elements at 12 years of age, his works on Conic Sections at 17 (published 1640), his arithmetical machine (the predecessor of all arithmometers) at 19, his creation of analytical geometry and the theory of probabilities (1654), and his final researches on the cycloid are familiar episodes in the scientific career of Pascal and form the chief subjects of M. Perrier's commentary. Pascal was a mathematical genius with a strong philosophical bent, who, by the accident of a feeble physique and a mental constitution made morbidly religious by his neuropathic condition, abandoned his early scientific pursuits, became one of the most notable apologetes of Christianity, the greatest controversialist of his age, and wrote a standard for the fixation of French prose.

The volume of M. Hatzfeld, like many French books during the last decade, has been sent out into the world a cripple—it has no index.

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A Further Record of Observations of Certain Trance Phenomena. JAMES HERVEY HYSLOP. Proc. Soc. Psych. Res., Pt. XLI., October, 1901, pp. 1 to 649.

In this report of 649 pages the author presents the results of his own investigations into the case of Mrs. Piper, together with an exhaustive analysis of the data and a discussion of their theoretical bearing. It may be well to state at once that he decides in favor of the spiritistic interpretation of the phenomena. Approximately the first half of the work is devoted to a discussion of the rival theories of telepathy and spiritism, this being preceded by a systematized abstract

of the records. The second half is made up of several appendices which contain the detailed records of the twelve sittings held by Professor Hyslop himself, together with those of the five sittings held by Dr. Hodgson for the former during his absence in New York. It is much to the author's credit that he urges upon the reader the necessity of making a careful study of these unabridged records before forming his opinions. Certain it is that many of the passages are far less convincing when read in their original context than when transplanted into Dr. Hyslop's zealous defense of spiritism. In the appendices are further recorded the results of some experiments in which the author endeavored to parallel certain phases of the Piper phenomena, by means of telegraphic messages and by talking through a speaking tube. These cannot be said to throw much light upon the subject.

As in all previous reports of the Piper case, facts are offered which seem, *prima facie*, to be conclusive evidence of some supernormal faculty or faculties on the part of the 'medium.' The present work differs in one respect from the earlier ones, in that here the 'communications' were all recorded by the medium's hand in writing ('Rector' control) instead of being uttered by her orally ('Phinuit' control) and recorded by one of the sitters. Thus one possible source of error is eliminated.

Of conscious fraud as an explanation of the phenomena in this case, Professor Hyslop remarks that he regards it "as having been excluded from view as much as ten years ago, and no one except those who have resolutely remained ignorant of the Society's work in general * * * would compromise his intelligence with that accusation without giving specific proofs of it." We may agree with the author in rejecting the fraud hypothesis as a probable one, but it is not likely that most psychologists will make the admission that it has been altogether 'excluded from view.'

But there is another possible explanation of many of these phenomena which would not carry us into the realm of the supernormal. This is that suggestions and indications are unconsciously given by the subject to the medium, the latter being in a hyperæsthetic and highly suggestible condition. 'Muscle reading' was rendered impossible in Professor Hyslop's experiments by the avoidance of physical contact between himself and the medium. Of verbal suggestion he says: "There are a few isolated instances, to which I have called attention in my notes and remarks as occasion required, in which suggestion is a conceivable explanation. But these are too few to allow them any weight in the whole, which the reader can easily see is unaffected by

such suspicions. * * * They are too infrequent to justify the waste of time and space in their examination" (p. 247).

But the author has certainly given far too little weight to such suggestions. Let us note a few of the 'isolated instances.'

"(Have you seen mother?) She is here with me. She is all right. * * * (Yes. Right.)" P. 309.[1]

The above question, addressed to his 'Brother Charles,' obviously implies that their mother is 'in the spirit.' The rest follows as a matter of course. Again—

"Do you remember McCollum [?] (S.: McAllum?) (R. H.: McCollum?) (S. to R. H.: No. I know what it is.)

"(Spell it again.) McAllum. (How was he related to you?) [Sic!] He was McAllan [?]. (Yes, that's it.) Don't you U D who I mean? He came over some time ago. [Correct, if it refers to my cousin.—J. H. H.] (Yes. I remember. Tell.)"—(P. 422.)

The 'communicator' resumes, after speaking of other matters.

"Yes, and McAllan. Well, you must know him. I had a cousin by that name. * * * [He was my cousin, not father's.—J. H. H.]" (P. 423.)

At the end of the sitting Professor Hyslop mentions the true name McClellan and it is interesting to know that in the next sitting the name is spelt almost correctly, viz., 'McLellen.' Also (p. 427), "I am your cousin H. H. McAllen." (Real name R. H. McClellan.)

The simple assertion that "at the end of the second sitting the name and relationship of my father was given as Mrs. Piper came out of the trance" is hardly a fair statement in view of the recorded facts:—

"[Mrs. P., as she was coming out of the trance, began to utter a name. I recognized this as 'Hyslop' twice before Dr. Hodgson, and deliberately refused to say so with the hope that he would recognize it also. * * * But as soon as I indicated what she was trying to say, doing this first by asking him, 'Don't you hear what she is trying to say?' and then saying to him 'Hyslop' (short sound of 'y'), he saw and assented at once, and Mrs. P. then pronounced the name much more distinctly. * * *]"—(P. 322.)

The author writes:

"His own name and mine were correctly given and it was he who first mentioned Robert and eventually Frank and Hettie as among his

[1] In the printed records, the utterances of the 'sitter' are put into parenthesis, comments and explanations into square brackets. 'S.' stands for 'sitter,' *i. e.*, Dr. Hyslop.

children. I mentioned George myself first (with the intention of misleading the communicator), and other communicators mentioned the rest of the children, Margaret, Sarah, Annie, Charles, Will and Lida before my father did so. The distinction was correctly indicated in all of these names between the living and the dead." (P. 86.)

This sounds very convincing. But one's conviction is shaken by passages such as the following:

"Now I have not spoken of Abbie yet. * * * (Abbie is not quite right.)

"Addie, no, did you say no? (That is not quite right.) [Repeated.]

"A * * * Nabbie. (R. H.: is that *Nabbie?*)

"A b sounds like Abbie, is it Addie?

"(What relation is that to me?) She is his sister.

"(Do you mean *Annie?*) *No.*

"(Oh, well, I know. I know who you mean now. Yes. I know who you mean now. But it is not spelled quite right.)

"He seems to say * * * let me hear for you Rector. [Apparently by G. P.]

"H Abbie. (The letter H is right.)

"Yes, but let me hear it and I will get it.—G. P.

"Hattie. (That is very nearly right.) Harriet.

"(Pretty nearly. Try it one letter at a time.)

"H E T T I E. G. P. (That is right. Yes. That is right and fine.)

"Ett [?] Hettie.—G. P. [Cf. 'McLellen G. P.' P. 429.]

"Yes, do you hear it, James? (Yes. I hear it.)"

This sister's name was Henrietta though she was never called 'Hettie' by the family as far as the writer knows.—(P. 434.)

It is fortunate, indeed, that the complete records are acccessible to the reader, for passages like the above arouse some mistrust of the author's competence to interpret them fairly. At times, indeed, he seems to have thrown scientific caution completely to the winds. The description of his death by his father's alleged spirit is characterized as 'one of the most remarkable though confused messages in the record'!

"Oh, yes. I hear. I hear you. Yes. I know now. Yes, my stomach.

"(S.: Yes. Was there anything else the matter?)

"Yes. Stomach, liver. (R. H.: Liver?)

"He says and head.

"(S.: Very well. Tell all about it.)

"He has taken off this condition, but tells me he could not see clearly. What was meant by his eyes? His stomach and * * *

"Speak plainly. * * * [To invisible.] I do not get it.

"Sounds like Bone [?] (R. H.: Can't read that.)

"(S.: Is that *bone?*) Bone [?] Bone [?] he * * * he is telling me. Wait.

"He places his hand over his * * * heart beat [?]

"(S.: Heart?)

"Yes, let me reach thee [not read] reach thee, friend.

"[Hand moves over R. H.'s head.]

"Think I am finding it hard to breathe * * * my heart, James * * * my heart, James * * * difficult to breathe.

"Do you not remember how I used to breathe? (S.: Yes, father, you are on the right line now.)

"Yes, I think it was my heart which troubled me most. * * * I * * * and my lung. * * *

"Stomach and heart. I felt a [undec.] and tightness of my chest * * * and my heart failed me. He says distressed in the region of the heart, but at last I went to sleep. Was it not congestion, James?

"(S.: Not that I know of.) [I had the catarrh in mind in this answer. I should have had the death scene in view. (November 3, 1899.)—J. H. H.]"—(Pp. 327–328.)

'Remarkable' this truly is in view of Professor Hyslop's statement (p. 26) that his father died of 'something like cancer of the larynx.'

A growing tendency on the author's part to adjust his facts to his theories at any cost becomes evident when we compare his earlier and later notes upon the same incidents. Whereas in the former due skepticism is commonly observed, in the latter the veracity of the 'communications' is saved by the most strained and unnatural interpretations. It is truly amazing with what ingenuity the author inverts and rearranges some of these utterances so that they may conform to fact. For instance, during the sittings conducted by Dr. Hodgson in Professor Hyslop's absence, statements are freely made by 'communicators' which appear to any unprejudiced reader to be the grossest fabrications. Such, for example, is the remark by the alleged 'father'—"I am thinking of the time some years ago when I went into the mountains for a change with him, and the trip we had to the lake after we left the camp." (P. 371.)

Of this the author himself writes in an earlier note: "It would require a great deal of twisting and forced interpretation to discover any truth in the statements for any one in the acquaintance of my father, even if it could be done in any way at all. It might suggest something to others, but it suggests only what is false to me." (P. 371.)

In a note added later, however, he propounds a most astounding hypothesis, accounting for the mistakes by a failure of 'Rector' (the 'control' who usually acted as amanuensis) to properly hear the message as dictated by the 'spirit.' "In order that the reader may see how nearly the passage is to being absolutely correct I may be allowed to reconstruct it somewhat with the imaginary confusion that ends in 'mountains' and 'camp.' If we assume anything like the trouble that was manifest in the guitar incident (cf. p. 461) the following is conceivable. 'I am thinking of the time some years ago when I went into [Father says 'Illinois.' Rector does not understand this, and asks if he means 'hilly.' Father says, 'no'! 'prairies.' Rector does not understand. Father says, 'no mountains.' Rector understands this as 'No! Mountains,' and continues] the mountains for a change with him, and the trip we had to the lake after we left [Father says, 'Champaign.' Rector understands 'camp,' and continues] the camp.'" (P. 409.)

And in reference to a mention of the name 'Bartlett' (no such person being known by the 'spirit' during life) the author's suggestion is that his father might be referring to *Bartlow* Township where one of his sons lived, or possibly to 'Bartlett pears of which father was very fond'!!

Such evidences of extreme bias may seem to many readers to quite discredit the value of Professor Hyslop's work in this field. And I cannot but think, with regret, that the publication of this Report may weaken, rather than further the cause of psychical research. However, after making full allowance for credulity and lack of caution on the author's part, it seems to me that he has added considerable to the already strong evidence in favor of the 'supernormal' character of the Piper phenomena. Whatever we may conceive the process to be by which the knowledge is obtained by the medium, many of us, with Professor James, 'cannot resist the conviction that knowledge appears which she has never gained by the ordinary waking use of her eyes and ears and wits.' There are numerous instances in Professor Hyslop's record of correct statements of fact given by 'communicators,' which it is very hard to attribute to fraud, suggestion or mere coincidence. A few such cases might be profitably cited.

The reference by the *soi disant* spirit of Professor Hyslop's father to *putting an organ into the church*, in reply to the remark by his son 'you will remember Harper Crawford, I think' (pp. 491–492), is certainly remarkable, in view of the facts of the case. This Harper Crawford was one of three persons who had left the church which Mr. Hyslop had attended, owing to the introduction of an organ, they having religious scruples against the use of instrumental music during worship. Dr. Hyslop did not know of these facts at the time of receiving the 'communication,' though it is not certain that he had not heard of them at the time. An examination of the previous records show no possible suggestions or clues which the medium could have used in this case.

Another instance—Professor Hyslop learns on May 16th that a family connection of his, named John McClellan, had died on the 30th of March, 1900. Being in New York at the time, he wrote to Dr. Hodgson, who was then conducting a series of sittings on his behalf, asking him to inquire at the sitting of June 4th if his (Hyslop's) father 'had any knowledge of anything recent to tell me.' Dr. Hodgson himself was not informed of the facts. The reply (not immediately given) is—

"And Mr. McClellan has come over to me and * * * splendidly * * * he is delighted with the change,[1] per * * * (Yes. *Which* McClellan?) John * * *" It was stated also that he was brother of 'James' McClellan and either 'uncle' or 'great-uncle' to James Hyslop. James McClellan was Hyslop's uncle by marriage; his brother John was the one that actually died on the date mentioned. Their father was also named John; hence, perhaps, the confusion between 'uncle' and 'great-uncle.' (Pp. 471, 472, 473.)

On one occasion the 'father,' who had enumerated at various times most of the members of the family, asks:

"Have I overlooked any one, James? * * * (Yes, you have overlooked one * * *)." (P. 441.)

In a later sitting, an 'uncle,' after discussing other matters, suddenly introduces the name 'Lida.' The following dialogue ensues:

"(Yes, I remember Lida. What relation is she to me?)

"Annie and she are cousins, Lida Aunt. (Yes, which Annie is cousin of her?) There is a sister Annie and a cousin Annie and Aunt Lida. She was an aunt to James Hyslop, if I remember rightly, and there is a sister in the body by that name. (Yes. Yes.)" pp. 459–460.

[1] This recalls 'Phinuit's' statement that 'It's a damn sight better here.'

The 'father' at this point interposes with the remark: "Which is the one I failed to mention."

The facts are these: Dr. Hyslop has a sister called 'Lida'; his aunt after whom his sister was named was called 'Eliza'; his uncle always abbreviated the name to 'Liza.'

In explanation of these phenomena, Professor Hyslop adopts unreservedly the spiritistic hypothesis. It is not likely that his arguments will convince any one who is not already prepared to accept this view. They are in part positive arguments for the spiritistic theory, in part objections to the alternative theory of telepathy. Some of these latter are certainly full of force. The evidence of some of the present records, and more yet of certain previous ones, goes to show that if telepathy occurs at all in this case, it must occur not only between the mind of the medium and that of the sitter present, but between the mind of the medium and that of some distant person who may be quite unknown to her. Of this supposition the author writes:

"To state it as boldly and clearly as is possible, it involves the power of the medium, wholly unconscious and not knowing the sitter, as any condition of establishing rapport at any distance, to select any absolutely unknown person necessary, anywhere in the world, and from his memory make the selection of pertinent facts to represent the personal identity, as that selection has been described for the mind of the sitter"!! (p. 139), and elsewhere:

"We may well halt before asserting or assuming such an omniscient power. * * * We may well ask, in reply, whether such a conception is not convertible with pantheism, or that form of monism that conceives all phenomena whatsoever, present, past and future, as modes of the absolute, a conception which I must consider as equivalent to spiritism, because we can as well postulate the continuance of each set of facts in this way as in the form of individualization usually imagined in the 'spiritual body' or immaterial soul." (Pp. 133 and 134.)

Against the often suggested analogy between telepathy and electrical induction, and especially the comparison with wireless telegraphy, he says:

"As a more conclusive objection to both this assumed analogy and to telepathy itself without that analogy, I may refer to the universal law of the distribution of energy in the physical world. This law is that force varies inversely with the distance; the ratio may be the square, cube or other power. This makes it possible to assign definite limits to the perceptible influence of such forces. * * * Her facts

are *selected* pertinently to her object without regard to space limitations, or the laws for the propagation of physical energy. Nobody seems to have any influence upon her 'subliminal' but the right person in the world, and that person unknown to her." (Pp. 140, 141.)

Certain positive arguments brought forward by the author for the spiritistic hypothesis are:

"(1) The unity of consciousness exhibited by the communicators, or the satisfaction of the criterion for personal identity. (2) The dramatic play of personality. (3) The mistakes and confusions. (4) Certain mechanical and coincidental features in the automatic writing of the medium." (P. 158.)

Regarding this 'unity of consciousness' he says:

"The whole organization of the synthesis is independent of the mind of the sitter, as they are not wholes of his past personal experience in the form in which they are presented as messages, but would have to be selected individually as elements and interwoven into the accurate true incidents that they are by a power which is infinitely vaster than anything we know in the physiology and psychology of both normal and abnormal phenomena." (Pp. 175, 176.)

The 'dramatic play of personality' is certainly a most interesting and striking feature of the case, even to those who believe, as most psychologists will, that we have to do at the most with phenomena of secondary personality. The coming and going of the various *dramatis personæ*, the faithfulness with which each adheres to his own part, never confusing his rôle with that of another, and the general, though not universal, consistency and lack of contradiction throughout the whole piece is certainly impressive. Though in general the various 'spirits' address the sitter, they occasionally engage in what Professor Hyslop calls 'transcendental conversations' among themselves, the record of such dialogues appearing, however, on the paper. A passage which admirably illustrates this dramatic character of the records, as well as the not infrequent humorous touches, is that relating to the name of Professor Hyslop's stepmother. The latter was always called in life 'Maggie,' her name being Margaret. The 'father' in these communications frequently mentions an 'Annie,' seeming to refer to this stepmother. Hyslop tries to get the true name stated but without success. He finally appeals to 'G. P.' who promises to get it for him. Much later in the sitting the following interruption occurs:

"I will speak for a moment, and say I do not see any reason for anxiety about Margaret. [Correct name of my stepmother.—J. H. H.]

"(R. H.: Who says this?) George.

"(S.: Margaret is right. The rest of it. Margaret is right. Can you tell the rest, George?)

"He said I suppose I might just as well tell you first as last and have done with it, or James may think I do not really know. Go tell him this for me. You see I got it out of him for you, H., but you no need to get nervous about it, old chap." (P. 486.)

It seems probable that Professor Hyslop has underestimated the histrionic possibilities of the secondary personality. The faithfulness with which a hypnotic subject will adhere to an imaginary rôle makes it at least credible that the trance consciousness, or consciousnesses, of the medium, evidently striving to their utmost to achieve this result, might be capable of an even greater skill in simulation. And indeed the earlier records, under the 'Phinuit' régime, suggest an elaborate piece of acting.

The author's arguments which are based upon the enormity of the assumptions underlying the telepathic hypothesis are fully met by the observed cases of 'cross telepathy' described by Andrew Lang (*Proc. S. P. R.*, Vol. XV.) granting, of course, that the latter be trustworthy.

Decidedly the weakest part of Professor Hyslop's work is his attempt to meet the objections to the spiritistic hypothesis. He seems to have ignored the formidable array of difficulties presented by Mrs. Sidgwick (*Proc. S. P. R.*, Vol. XV.) based upon a study of this very case. It will surprise those who have read some of the extracts which I have cited from Dr. Hyslop's records to hear him declare that:

"The first thing to be said in regard to the difficulties and objections to the spiritistic theory is that, from the standpoint of my own sittings alone, there are no serious obstacles to the doctrine. If I had to judge the case by my own experiments and record alone, I do not see how I could avoid the conclusion that a future life is absolutely demonstrated by them." (P. 242.)

It is true that some of the most cogent objections to the theory are based upon previous reports of the Society. But an abundance of facts are to be gathered from the records of Professor Hyslop's own studies that will certainly strike the unprejudiced reader as being 'serious obstacles.' During Dr. Hyslop's absence, Dr. Hodgson made a very clear statement to the alleged spirit of the former's father, describing the conditions of the sittings and the purposes of the experiment. The 'spirit' expressed his entire appreciation of the situation, and promised to do as well as his memory would permit in complying

with the tests. In spite of these promises, the subsequent statements are as confused and as far from the truth as before. The anecdotes recalled by the 'father' as especially good 'tests' are not verified by Professor Hyslop upon inquiry of members of his family who ought to have known. The case recalls that of 'Hannah Wilde's' letter, as cited by Professor James (*Proc. S. P. R.*, Vol. VI.) and by Dr. Hodgson (Vol. VIII.), and that of the names of Stainton Moses' 'controls,' as narrated by Professor Newbold (Vol. XIV.). In these latter cases, the alleged 'spirits' made long circumstantial statements about matters of fact with which they ought to have been quite familiar, but their accounts proved to be complete fabrications.

Again, expressions are freely used by 'communicators' which were not in the least characteristic of the persons when living. Professor Hyslop's 'father' uses the word 'Sunday' in place of 'Sabbath,' contrary to a lifelong habit; he also refers to his sitting-room as his 'library,' his carriage as his 'coach,' and makes mention of his 'diary,' a thing which, in reality, he never kept.

But it is when we look into some of the previous records of the Piper case that we meet with the most insuperable objections to the theory which Professor Hyslop defends. It is quite conceivable that a man's near relatives and friends should send him messages, providing that a way were opened, but what shall we say of the appearance upon the scene of Mrs. Siddons, Sebastian Bach, Commodore Vanderbilt, Ulysses, Homer and *Adam Bede?* None of the spirits have presented better credentials than 'George Pelham.' If his identity be disproved, the evidence for that of no other spirit can be said to have much weight. And yet this very George Pelham vouches for the identity of 'Phinuit,' although the latter gave most contradictory accounts of himself, and he even corroborates the contention of 'Doctor' that he is Homer! [1] What can be said of George Pelham himself under the circumstances? And how are we to explain the appearance of the 'spirit' of the author of a manuscript which was presented to Mrs. Piper during her trance, when the author himself was alive and well? Objections of equal force might be multiplied almost indefinitely.

Many of these objections are not considered at all by Dr. Hyslop. The others are met by resorting to such assumptions as, firstly, that

[1] Of the chief among Mrs. Piper's present 'controls,' Professor Hyslop remarks: "Imperator's temper represents, in its philanthropic sympathy for man, as nearly as anything I know, the character and purposes of Jesus Christ." (P. 181.) It is incredible that he really means what the reader might easily infer from these words.

serious difficulties exist in the way of communication through the medium; and, secondly, that the 'communicator' himself is not in a normal mental condition at the time, being in something like a dream consciousness during the process of communicating. To all objections based upon the incredibility of spirits existing under such conditions as they sometimes describe for themselves, he replies with the assertion of our complete ignorance of the conditions of a discarnate existence, and the consequent injustice of judging the case by any preconceived standards. We doubt whether Professor Hyslop himself is really satisfied with such a disposition of the numerous glaring absurdities contained in the accounts we get of the 'other side.'

We should certainly avoid some enormous difficulties if we could assume that in the Piper case we had to do with an exceedingly complex instance of multiple personality, each phase of which was convinced of its identity with the individual it purported to be, and acted its part accordingly. The information given is certainly derived in great part from suggestions received from the sitter, these being seized upon, apparently, by an abnormally keen perception and stored away by an abnormally tenacious memory. But there is a considerable residuum of facts that it seems very difficult to explain without invoking some supernormal faculty as yet very little understood. To call this 'telepathy' or 'clairvoyance' or by some other name would not help us to a solution. Words are not explanations. We await more data.

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It seems desirable to publish in the REVIEW a somewhat detailed examination of the last part of the *Proceedings of the Society for Psychical Research*, and it seems best that it should be prepared by an author taking a sympathetic attitude toward the phenomena with which the Society is concerned. It should not, however, be assumed that this attitude is shared by the editors of the REVIEW. The phenomena are apparently not such as can be treated by scientific methods, and each must form an individual opinion. The undersigned believes that the facts reported do not require or justify the assumption of the supernormal as a working hypothesis. Neither does he regard the phenomena as suited to scientific investigation until the possibility of fraud has been excluded.

J. MCKEEN CATTELL.

ASSOCIATION.

Experimentelle Untersuchungen zur Associationslehre. FRIEDRICH SCHMIDT. (Aus dem psychologischen Institut der Universität Würzburg.) Zeitschr. f. Psychol. u. Physiol. d. Sinnesorgane, Bd. 28, Heft 2, pp. 65–95 (1902).

This research was suggested by, as it is a further elaboration and criticism of similar investigations by Thumb and Marbe ('Experimentelle Untersuchungen über die Psychologischen Grundlagen der Sprachlichen Analogiebildung,' Leipzig, 1901). The subjects of the experiment were children, from the 'Würzburger Stadtschule,' who had not yet been drilled on the grammatical paradigms.

The words were called out to the children and the answers (first associations) and the time recorded. Both verbs and adjectives were used. In general, Marbe's law was confirmed, that the reaction is quicker in direct ratio to the familiarity of the words associated, but Schmidt did not find Marbe's other law confirmed, that the average time of the larger groups of identical reactions was shorter than the average time of the smaller groups.

The order of preference in associations, in the case of verbs, he found to be: (1) Some form of the same verb or of some other verb, (2) substantives, (3) adjectives, (4) other parts of speech. This agrees with the results of Thumb and Marbe except that (1) and (2) are reversed. This difference, as well as the difference between the results of his own researches and those of his predecessors in the case of adjectives, Schmidt lays to the slightly different methods employed.

Ten tables set forth the results of the investigation in an instructive way, but many more investigations will have to be carried out along this line before the author's hope will be justified that this sort of inquiry will be of much value to the philologist.

An interesting part of the research is the comparison made between the different types of 'gegenseitige Reactionen' in the case of both verbs and adjectives, and especially the strong tendency in certain subjects to construct unusual antithetic adjectives such as *unzukünftig*, *unvoll*, *unewig*, etc. H. HEATH BAWDEN.

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THE PERCEPTION OF SPACE.

Zum Problem der Grundlegung der Tiefenwahrnehmung. A. KIRSCHMANN. Philosophische Studien, XVIII., 114.

This paper is an attempt to substantiate the theory propounded by the author in 1894 in the same journal under the title: 'Die Paral-

laxe des indirecten Sehens und die spaltförmigen Pupillen der Katze.' The occasion for this substantiation is to be found in an article by R. Müller, published in the *Studien* for 1898, in which the theory of Kirschmann is unfavorably treated on both theoretical and experimental grounds. Let us briefly recall the chief points under discussion.

From a consideration of the geometrical factors involved in vision, Kirschmann showed that the retinal images of points lying at different distances and seen in indirect vision must undergo peculiar shiftings whenever there are changes of accommodation or movements of the eye in its socket. In the first place the images keep their position relative to one another, but shift towards or away from the fovea, according as the accommodation changes for a nearer or a more remote point. In the second case the images undergo displacement relative to one another, coming together or moving asunder as the point of regard approaches or recedes from the objective points in question.

Now it is just these shiftings of images on the excentric parts of the retina, so Kirschmann claims, that give to *monocular* vision an extremely important factor in the perception of depth. To be sure, they are not consciously noticed, but no more are ordinary double images which exist unseen for perhaps the majority of individuals, though they are the indispensable condition of stereoscopic vision.

This theoretical deduction was supported in the main by two lines of argument. First, the theory explains clearly why changes in the size of the pupil accompany changes of accommodation and convergence. This is not a question of the amount of light admitted to the eye, but rather a question of the sharpness of the images upon the outlying parts of the retina. For these images, coming as they do from objects at varying distances, are really dispersion circles—projections, that is, of the pupil upon the retina. In the interests of clear monocular perception of depth, then—and such perceptions must and do take place in the lower and outer portions of the field of view—these dispersion circles must be as sharply defined as possible, a condition that is secured by a contracted pupil. And since, further, the parallax of indirect vision is effective for the near ranges of vision only, convergence and accommodation for near objects must be accompanied by a narrowing of the pupil. In the second place, the slit-like pupil of the cat is accounted for by similar considerations. For the crouching mode of hunting requires that the perceptions of relative depth made in indirect vision be exact along the longitudinal meridian only. This is secured by a pupil narrowing to a vertical slit.

R. Müller pretends to bring the above theory to an experimental test. He also urges objections of various sorts. It is with these experiments and criticisms that the article before us is concerned. As to the former, Kirschmann easily shows that they are entirely irrelevant to his own claim, since they are concerned wholly with the capacity to establish an equality of distance between a point seen monocularly in indirect vision and the point of fixation. To be relevant to the author's theory they should have investigated the ability to perceive the relative distances of objects in the lower lateral parts of the field of view and at distances within the reach of the hand.

The author admits very freely that in his opinion his hypothesis is not accessible to direct empirical proof, since the factors involved can hardly be brought to the focus of attention. Still indirect proofs are not wanting. Two of these have been mentioned above as discussed in the article of 1894. A third, to which attention is here again directed, is to be found in the author's article : 'Der Metallglanz und die Parallaxe des indirecten Sehens,' published in the *Studien* in 1895.

As to Müller's theoretical criticisms, the main one is that the magnitudes concerned in the parallax of indirect vision are too insignificant to form the basis for perceptions. This objection had been anticipated by Kirschmann in his first article, and a table of calculations was there given which showed that even under the most unfavorable conditions the values assigned to the parallax could not be looked upon as imperceptible. To strengthen this claim and remove the edge from the objections mentioned the author describes some interesting new experiments involving chromatic aberration. Colored lines and figures on dark or light backgrounds, as the case may be, are viewed binocularly through a large reading-glass. The refraction is too slight to cause a blurring of the lines at the edge of the field, yet it is great enough to make itself felt in marked depth displacements in the figures. If a dark background be used, red and blue are the best colors to use. In this case the red lines advance into the foreground. With a light background blue and yellow give the most striking results. When these are seen through the lens it is the blue that approaches the observer. With these results before one, the author thinks that no one should any longer hesitate to accept his hypothesis for the reason that it deals with insignificant magnitudes.

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HEARING.

L'Audition. Pierre Bonnier. Paris, O. Doin. 1901. Pp. 276.

This volume forms part of the 'Bibliothèque Internationale de Psychologie Expérimentale,' edited by M. Toulouse. The subjects treated are as follows: The evolution of the ear is sketched in an illustrated chapter. The author denies a sense of hearing proper in invertebrates and fishes, that is to say, in all animals not possessing a cochlea. The function of the more primitive 'ears' is the perception of pressure and of oscillatory variations in the surrounding fluid medium, or of the trembling of solid objects. The anatomy of the human ear is presented in a series of excellent figures. Physiology is represented by an extended discussion of the different theories regarding the action of the cochlea in the reception of sound-waves. The author rejects Helmholtz's theory and all similar theories, and defends one already advanced by himself, which differs from the rest in denying that the fibers of the basilar membrane, or any other cochlear structure, act as resonators and vibrate sympathetically and separately with sounds of different pitch. His objections to the theory of resonators are: first, that no organs exist in the inner ear that are large enough to vibrate in unison with low tones, or with any but the highest audible tones; and, second, that the difference between the largest and smallest supposed resonators is by no means proportional to the difference between the wave-lengths of high and low tones. According to the author's theory, every element of the cochlea is capable of stimulation by waves of any length. The multiplicity of elements, since many of them will be stimulated almost but not quite simultaneously by a given lymph-wave, serves to convert a vibratory movement of the lymph into a continuous activity of the auditory nerve, and so gives rise to the continuous sensation of sound. Another part of the author's theory is that the movements of the solids and liquids of the inner ear are molar, not molecular vibrations. The movement impressed by the stapes on the perilymph of the vestibule travels by the shortest route, through the membranes, to the round window. In depressing the basilar membrane, it pulls on the hair processes of the cells of Corti and stimulates the nerve-ends.

The more strictly psychological topics treated comprise:

The perception of *timbre*. This does not consist in an analysis of a tone into its fundamental and harmonics; what is heard is not the component vibrations, but the resulting *form* of the total vibration.

The localization of sound: The author draws a sharp distinction between the feeling of the direction of sound, which is present in mon-

aural hearing, and the feeling of auditory depth or distance, for which the basis is provided by the different impressions made by a sounding body on the two ears.

The theory of music: This is very briefly treated. A sound is disagreeable if it is so complicated as to require great effort for its appreciation. The 'law of least effort' applies here as in other realms of æsthetics. The degree of effort attending the appreciation of a sound does not depend entirely, however, on the complexity of that sound alone, but also on the preparation that has been made for it by immediately preceding sounds. The musical scale has been built up on the principle of easy passage from one note to another. There is need in music, as in other forms of æsthetic attention, of frequent periods of relaxation, *i. e.*, in the present case, of frequent modulation from complicated chords to simple ones.

The clinical side of the subject is represented by a discussion of the different methods in vogue for measuring the acuity of hearing, by an untechnical account of the causes of deafness and other disturbances of hearing, and by a chapter on the early diagnosis of progressive deafness.

The author has confined his book to subjects in which he has himself been an investigator. Hence a certain freshness, and also a certain controversial air in his style of treatment. Hence, also, the book is by no means cyclopedic in scope. R. S. WOODWORTH.

UNIVERSITY AND BELLEVUE HOSPITAL
MEDICAL COLLEGE.

SLEEP.

The Necessity for a New Standpoint in Sleep Theories. HENRY HUBBARD FOSTER. The American Journal of Psychology, Vol. XII., 2, January, 1901.

This is a useful résumé, accompanied by a criticism, of the various and sundry theories of sleep, to which are added some suggestions upon future lines of inquiry. Of the *circulation* theories, we have the old 'brain congestion' theory, the more modern 'brain anæmia' theory of Durham, Hammond, Howell and others, and certain recent theories of cortical anæmia with hyperæmia at the base of the brain. Future theories must reckon with the fact of at least cortical anæmia, but will probably regard it as a symptom and not the cause of sleep. The *chemical* theories are divided into the combustion theories and the auto-intoxication theories. As regards the first, the author thinks it unwarranted to attribute sleep, as Pflüger does, to decrease in gaseous exchange. As regards the presence of decomposition products in the

blood or cerebral centers, it is dogmatic to refer sleep to any particular toxic agents. Nevertheless, 'if the ultimate explanation of the phenomena of sleep is to be found anywhere, it must be sought in the chemical composition and the chemical changes occurring in the nerve cell.' Lastly come the *histological* theories, particularly those referring sleep to the partial paralysis of the amœboid prolongations of the neurocytes resulting in an isolation of the nerve elements.

The 'new standpoint' which the author urges is the evolutionary. Regarding sleep as the cessation of consciousness, we must inquire under what conditions consciousness has arisen in complex organisms. Such a study will show that the physiological conditions necessary to the support of consciousness will necessarily suffer periodic collapse due to fatigue, limitation of capacity, rhythmic habit, etc.

Although the author is working upon a somewhat different problem from that of the writers whom he reviews, still it is no doubt true that this broader standpoint would have saved some confusion in the discussion of the *causes* of sleep. A bibliography is appended.

G. T. W. PATRICK.

UNIVERSITY OF IOWA.

NEW BOOKS.

Neurological Technique. I. HARDESTY. Introduction by H. H. DONALDSON. Chicago, The University Press. 1902. Pp. xii + 183.

An exposition of various methods of 'preparation and study of vertebrate nervous tissues.' It contains also a valuable detailed list of anatomical terms, 'Anatomical Nomenclature for the Nervous System and Sense-Organs.'

Der Æsthetische Genuss. K. GROOS. Giessen, Ricker'sche Verlag. 1902. Pp. viii + 263.

The Philosophy of Conduct. G. T. LADD. New York, Scribners. 1902. Pp. xxii + 663. $3.50.

Psychologie du Rire. L. DUGAS. Paris, Alcan. 1902. Pp. 178. Fr. 2.50.

La Logique chez l'Enfant et sa Culture. F. QUEYRAT. Paris, Alcan. 1902. Pp. 156. Fr. 2.50.

Fragments in Philosophy and Science, being Collected Essays and Addresses. JAMES MARK BALDWIN. New York, Charles Scribner's Sons. 1902. Pp. xii + 389. $2.50.

Les Caractères. FR. PAULHAN. 2me Ed. 1902. Paris, Alcan. 1902. Fr. 5.

Differs from the first edition mainly by the addition of a long new preface in which the author answers objections to his views.

Analytical Psychology. LIGHTNER WITMER. Boston, Ginn & Co. 1902. Pp. xvii + 251.

Psychologie Économique. G. Tarde. 2 vols. Paris, Alcan. 1902. Pp. 446 and 384. Fr. 15 each vol.

Psychologie du Délire dans les Troubles psychopathiques. N. Vaschide and Vurpas. Paris, Masson. No date. Pp. 191.

Saint Anselme. Domet de Vorges. Paris, Alcan. 1901. Pp. vi + 334. Fr. 5.

A Syllabus of Psychology. H. Heath Bawden. Vassar College, Poughkeepsie, N. Y. 1902. Pp. 109.

Gehirn und Seelenleben. Th. Ziehen. Leipzig, Barth. 1902. Pp. 66. M. 1.50.

Mental Growth and Control. Nathan Oppenheim, M.D. New York, The Macmillan Co. 1902. Pp. vii + 296.

Dr. Oppenheim states some of the elementary facts about the structure of the nervous system, attention, association, instinct, memory, habit, suggestion, imagination, the emotions, reasoning and voluntary action, adds some personal opinions of his own concerning the nature of mental functions and uses both as the basis of earnest exhortations to the young to improve their mental efficiency. The book is not intended for academic use and is of no especial significance to investigators or teachers of psychology.

Der Hypnotismus. L. Loewenfeld. Wiesbaden, Bergmann. 1901. Pp. 522. M. 889.

An extended, clear, judicious, and admirable presentation of the subject of hypnosis and suggestion, intended as a hand-book. It is written with especial reference to the meaning of suggestion for the subjects of medicine and law. It contains an adequate historical chapter and a classified bibliographical list—which last, however, might be considerably extended, especially by the addition of the titles in English. The book is possibly the best résumé we have to date and an English translation would be of service. J. M. B.

L'Année Biologique. Yves Delage. 5me Année. 1899–1900. Paris, Schleicher Frères. Pp. lxxvi + 676.

This issue of this excellent annual covers two years and so endeavors to 'catch up.' It has the same admirable features as heretofore, nearly one-fourth being devoted to the 'mental functions.' The loss to the psychological department of the *Année*, through the death of Marillier, is very heavy. We understand that M. Philippe is to take a leading place in analyzing psychological works for the *Année* in the future. J. M. B.

Ueber die allgemeinen Beziehungen zwischen Gehirn und Seelenleben. Th. Ziehen. Leipzig, J. A. Barth. 1902. Pp. 66. Mk. 1.80.

Kant's Prolegomena to any Future Metaphysics. Edited in English by Dr. PAUL CARUS. Chicago, The Open Court Publishing Company. 1902. Pp. v + 284.

NOTES.

THE American Philosophical Association held its first meeting at Columbia University, New York City, on March 31 and April 1, 1902. For some years philosophical papers have been presented at a special session of the American Psychological Association, but the desirability of a special society has been for some time recognized, and this was formed at a conference held in New York last November. Professor J. E. Creighton, of Cornell University, was elected president, and Professor A. T. Ormond, of Princeton University, has now been elected his successor. The Association will meet next winter at Washington during Convocation Week with the American Psychological Association and other societies. The papers read at the recent meeting were as follows:

'Poetry and Philosophy': Dr. Ralph Barton Perry.

'Recent Criticism of the Philosophy of T. H. Green': Professor William Caldwell.

'The Æsthetic Element in Human Nature': Professor E. Hershey Sneath.

'Address of Welcome': President Nicholas Murray Butler.

'The Functional Theory of the Distinction between the Psychical and Physical': Professor H. Heath Bawden.

'The Atomic Self': Professor George Stuart Fullerton.

Address of the President. Subject, 'The Purposes of a Philosophical Association': Professor James Edwin Creighton.

Discussion on the Address: President Francis L. Patton.

'The Concept of the Negative': Dr. W. H. Sheldon.

'Being, Not-being and Becoming: a Study in the Logic of Early Greek Philosophy': Professor Alfred H. Lloyd.

'Aristotle's Theory of Reason': Professor William A. Hammond.

'On Final Causes': Dr. Edgar A. Singer, Jr.

'On the Study of Individuality': Professor J. A. Leighton.

'The Conciousness of Obligation': Professor E. B. McGilvary.

'Kant and Teleological Ethics': Professor Frank Thilly.

'Epistemology and Ethical Method': Dr. Albert Lefevre.

'The Epistemological Argument for Theism': Professor Edward H. Griffin.

'The Philosophy of Religion: Its Aim and Scope': Dr. F. C. French. (Read by title.)

Professor F. J. E. Woodbridge, now head of the department of philosophy in the University of Minnesota, has been elected to a chair of philosophy at Columbia University in view of the election of Dr. Nicholas Murray Butler to the presidency. Dr. Butler retains the chair of philosophy and education, and offers two courses in philosophy next year. Professor Cattell has been made head of the department of philosophy and psychology.

Dr. Chas. H. Judd, professor of psychology in the University of Cincinnati, has received a call to Yale University.

Dr. Frederick W. Colgrove has resigned the professorship of philosophy in the University of Washington, being seriously ill.

Professor Josiah Royce, professor of philosophy at Harvard University, and Professor J. Mark Baldwin, professor of psychology at Princeton University, will lecture before the summer school of the University of California during July.

Professor James is at present abroad, in order to give his second course of Gifford lectures at Edinburgh University. Professor James and President Schurman have been given the LL. D. degree at Edinburgh.

Dr. Gwatkin, professor of ecclesiastical history in the University of Cambridge, has been appointed to succeed Professor James as Gifford lecturer at Edinburgh; Professor Emile Boutroux, of the Sorbonne, has been elected Gifford lecturer in the University of Glasgow, in succession to the Master of Balliol.

Dr. W. H. R. Rivers, of Cambridge University, will shortly start on an expedition for the psychological study of the Todas of southern India on the lines of his work in Torres Straits.

Professor Leo Königsberger, of Heidelberg, is preparing an extended biography of Hermann von Helmholtz, which will be published by Friedrich Vieweg and Son.

In view of the special attention paid to psychology at Clark University, it is of interest to note that the will of the late Jonas Clark has been settled, and that the University will receive $2,600,000.

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THE PSYCHOLOGICAL REVIEW.

STUDIES FROM THE PSYCHOLOGICAL LABORATORY OF THE UNIVERSITY OF CHICAGO.

COMMUNICATED BY JAMES ROWLAND ANGELL.

EXPERIMENTAL OBSERVATIONS UPON NORMAL MOTOR SUGGESTIBILITY.

BY HAYWOOD J. PEARCE.

The present paper deals with the modifications which certain motor coordinations display under the influence of sensory stimulations of a specific character. The coordinations selected were all movements of localization. The results of the experiments seem to throw an interesting light upon the psychophysical conditions involved in such movements and they indicate the possible nature of some, at least, of the familiar phenomena of suggestion. There appears to be reason to hope that a practicable, even though rough, criterion of general constitutional, or organic, suggestibility may, perhaps, be established along the line of the methods here employed.

The experiments, which grew out of some observations upon the localizing of contact sensations, include, in addition to extensive tests upon the localizing movements of the left hand, when the skin of the right forearm is stimulated, two sets of tests upon eye movements, when these are employed to localize sounds and visual objects. Throughout the paper the emphasis falls purposely upon the qualitative analysis of the conditions involved and not upon the quantitative features of the situation. This statement is not to be interpreted as prejudicing the relia-

bility of such quantitative statistics as are reported. These are all regarded as trustworthy within the limits of the methods adopted. But had the quantitative problems been of fundamental interest, these methods would have been modified in several particulars. The investigation is being carried forward with other and more exact modes of procedure.[1]

The literature bearing most immediately upon the subject of the investigation may be classified under two heads. First, that relating to the effects upon the contractions of both voluntary and involuntary muscles of various psychical conditions and especially the effects of various forms of sensory stimuli. The literature under this head is too extensive to cite exhaustively. Illustrative, however, of the general field are Mosso's observations of the circulatory changes accompanying emotional excitement;[2] Féré's observations upon the shifting power of voluntary muscular contractions under the influence of agreeable and disagreeable stimuli;[3] Mitchell's, Lombard's and Bowditch's studies of the modifications which the knee-jerk manifests under stimulations of both sensory and motor character.[4] Summarizing the positive purport of all these investigations, so far as they bear upon the present experimentation, we may be said to know already that muscular contractions reflect in an extremely sensitive way, capable within limits of quantitative statement, the momentary conditions of the whole nervous system. The cases we shall have to study will be seen to constitute a specific set of instances under this general principle.

As a subordinate investigation under this general head may be mentioned an article by Münsterberg and Campbell upon

[1] I am under obligation to Professor James R. Angell for counsel and assistance throughout the course of the experimentation. My sincere thanks are also due the persons who so kindly served as subjects for me, to wit: Misses M. Graves, E. Bartlett, E. Pearce, J. Biggars; Messrs. J. R. Angell, E. H. Mahood, C. A. Woolsohn, W. C. Gore, J. H. Lathrop, L. D. Arnett, E. L. Kimball, R. Clark, M. Hall, R. C. Adams, W. A. McKeevor; Masters H. Pearce and T. Pearce.

[2] Mosso, Kreislauf d. Blutes im Mensch. Gehirn.

[3] Féré, Sensation et Mouvement.

[4] Mitchell, *Medical News* (Philadelphia), 1886 (Feb. 13 and 20).
Lombard, *Amer. Jour. Psy.*, I., p. 1.
Bowditch, *Boston Medical and Surgical Journal*, 1888 (May 31).

the motor power of ideas.[1] I have adopted in part of my experiments an apparatus similar in many particulars to that used by these writers and by a different method have confirmed what they sought to establish. By varying the method I have also secured new results, which afford considerable assistance in interpreting the Münsterberg-Campbell observations.

The second main group of papers in the relevant literature concerns the discriminative sensibility of the skin. In the tactual experiments I have adopted methods somewhat similar to those used by E. H. Weber and later by Volkmann, Henri, Judd and others.[2] Further than a general similarity of method our experiments have little, if anything, in common. Such occasional points as are significant for my investigation will be more conveniently referred to in connection with the report of the experiments.

The rapidly enlarging field of literature on the subject of suggestion has necessarily had a general influence in determining my attitude with reference to the interpretation of my observations. As most prominent, perhaps, in this respect may be mentioned Sidis' 'Psychology of Suggestion,' Schmidkunz's 'Psychologie der Suggestion' and Binet's 'La suggestibilité.'

Dermal-Motor Suggestibility.

The dermal experiments number over four thousand and were made upon six persons, four men and two women. The conditions were varied slightly from time to time, as will be pointed out later, but in general they were as follows. The subject, being seated, was required to bare his right arm to the elbow and place the same upon a low table or other support, palm upward, and in as comfortable a position as possible. The stimulations were distributed equally over the region of the mid-arm, the wrist and that near the elbow, being given in the order named. The subject's eyes being closed, a fairly vigorous pressure was made upon the arm with a blunt instrument

[1] Münsterberg and Campbell, Psych. Rev., I., p. 441. Cf. also the drastic criticism of this paper by Nichols, *Phil. Rev.*, IV., p. 174.

[2] Henri, L'Année Psychol., II., p. 295. Also, Die Raumwahrnehmungen des Tastsinnes. This monograph contains an excellent bibliography of the subject. Judd, *Philos. Studien*, XII., p. 409.

and the subject was required immediately to touch as near the same point as possible with a similar instrument held in his left hand. He was allowed only one trial, *i. e.*, the point where he first touched was the one noted, and the error both in amount and direction was recorded. The normal displacement, *i. e.*, the average error when a single stimulus was localized, was first determined. To determine this ten tests were made with a single stimulus in each of the three regions of the arm named—the stimulations being given first on mid-arm, then on wrist, then near elbow, the process being repeated ten times. Then the same process was repeated with the addition of a second stimulus 6.5 cm. below the first, given immediately following, or in the case of some subjects simultaneously with the one to be localized.[1] Thus a series of ninety tests was completed. The experiments at each sitting usually occupied one hour, during which time from ninety to one hundred tests were made.

Various simple devices were employed for giving the stimulations. In the majority of the experiments an ordinary pair of æsthesiometric compasses was used, the points of which are spherical and about a millimeter in diameter. The points are likely to set up slight temperature sensations in addition to the touch sensation and in some of the series of tests wooden points were substituted. For the purpose of the investigation this is really a matter of no great consequence.

In making up the tables, it will be observed, I have disregarded errors made to right and left of the point to be localized. These would only complicate the situation. When the localization was exactly right or left the error is regarded as zero; if the localization was down or up *and* to right or left, the error down or up only is considered. These errors to right and left have apparently no uniformity and I could not see that they were materially altered when the second stimulus was given. The amount of such lateral errors varied from zero to about twelve millimeters, but was generally nearer the former figure.[2]

[1] This distance is considerably in excess of that where the two points are felt as one by most observers.

[2] The practical justification of this procedure is supported by Lewy's investigations—*Zeitsch. für Psychol. u. Physiol. d. Sinnesorg.*, VIII., p. 231. He found these errors which I disregard as few in number and insignificant.

Only two of the subjects were aware of the purpose of the experiments. They were instructed to attempt as much as possible to disregard the second stimulus, *i. e.*, not to allow it, consciously, to influence them in either direction. In other words, they were required to make the reaction in as reflex a manmer as possible. The subjects who did not know the object of the experiments were told that the purpose of the second stimulus was to test the effect of distraction, to see if they could localize as well with as without a second stimulus. When questioned after all of the experiments had been completed, these subjects were still totally ignorant of the real purpose as well as the *result* of the experiments. All the subjects agreed that, so far as they could judge, the only effect of the second stimulus had been to render a trifle more difficult the identification of the point which they sought to touch. In order to test the accuracy of this testimony I had one of the subjects make a series of the tests on myself and I was unable to tell, until after I had examined the figures, what the nature of my own reactions had been. One subject had considerable difficulty in distinguishing the two stimuli as two when they were about six centimeters apart and given near the elbow. It was interesting to observe, however, that even under such conditions his reactions were practically as accurate as when the two points were clearly distinguished. This serves to illustrate the essentially reflex nature of these reactions.

In the experiments which have been recorded in the literature of this subject, the emphasis has, so far as I am aware, been placed upon the sensory side—upon the accuracy or inaccuracy of the sensory discrimination of the points stimulated. In my experiments this feature has been of secondary, even of incidental interest. I have worked upon the theory that every sensation and every image of a sensory stimulus has a more or less definite motor element of a kind tending to release movements of localization. In reflex action, for example, a motor impulse is somehow more or less definitely coordinated with a particular sensory stimulus. Disregarding the question of how the particular coordinations under discussion arise, it has been my purpose to determine the accuracy of

this motor impulse, and later, to determine how such impulses may be modified by other similar stimuli both successive and simultaneous. The subject, therefore, was not allowed to move the pointer after it had once touched the arm. This was frequently quite unsatisfactory to the subject and some subjects were anxious for further trial, believing they could do better. In many instances this was undoubtedly true—the subjects had a fairly definite idea of the direction and the amount of the error. But in many other instances they were quite satisfied with the result and in still other cases, when questioned or given permission to try again, their judgment was quite at fault. This last was particularly true if the first error was very small. On the whole, I think the tabulated results will show that the average errors in localization made by using this method are not very much greater than the errors recorded by others, who have allowed the subject to keep trying until satisfied; but it is not claimed that the figures represent in any degree the discriminative sensibility of the skin as a sense-organ. To be sure Henri comments on the great uncertainty attaching to our judgments of such localizing movements.[1] It is not entirely clear from his description how closely his conditions are comparable with mine. Apparently the localizations he refers to were made by pointing and not by effecting actual contact. This would certainly introduce an important difference. In any case the facts are as just stated. The difference in the accuracy of the localizations under the two modes of procedure seemed neither marked nor constant, and as the point to be investigated was the effect of the secondary stimulus upon the movement, rather than the sensitivity of the skin, the method adopted seemed entirely justifiable.

Results.

The first table presented (Table A.) shows the averages obtained from each subject. It will be noticed that with a single stimulus all of the subjects except one displace downward, *i. e.*, toward the wrist. V. Henri,[2] in his experiments, to which refer-

[1] Henri, Raumwahrnehmungen d. Tastsinnes, p. 114.
[2] 'L'Année Psychologique,' Vol. II., p. 177.

TABLE A.—TACTUAL.[1]

Subject.	Region of Arm.	Single Stimulus.				Suggestion Up.								Suggestion Down.							
		Tests.	Av. Error.	Direction.	M. Variation.	Tests.	Av. Error.	Direction.	M. Variation.	Per cent. of Pos. Influence.	Per cent. of Neg. Influence.	Positive Influence in cm.	Neg. Influence in cm.	Tests.	Av. Error.	Direction.	M. Variation.	Per cent. of Pos. Influence.	Per cent. of Neg. Influence.	Positive Influence in cm.	Neg. Influence in cm.
A	Forearm.	60	.78	D	.57	70	.20	U	.77	74	26	.80	.02	70	1.08	D	.61	61	39	.46	.26
B	"	114	.72	D	.54	119	.77	U	.79	82	18	1.46		118	1.20	D	.82	68	32	.63	.23
C	"	57	.54	U	.42	58	1.06	U	.44	79	21	.98		58	1.86	D	.43	98	2	1.93	
D	"	96	.84	D	.71	111	.31	U	.72	84	16	1.11	.08	98	1.05	D	.64	64	36	.61	.31
E	"	131	.73	D	.51	156	1.51	U	.61	93	7	2.02	.01	163	1.02	D	.59	56	44	.79	.53
F	"	150	.39	D	.64	153	1.37	U	.80	90	10	1.77		159	.44	D	.58	44	56	.38	.38
G	"	236	.07	D	.66	221	1.54	U	.75	89	11	1.62		219	.57	D	.61	67	33	.69	.26

ence has already been made, found that his subjects usually displaced in the direction of a 'point of reference,' which they had in mind in making the localization. This point of reference was usually a bone or some prominent configuration of the surface of the skin. If, however, this point of reference was a considerable distance from the point touched, the error was usually made in the opposite direction. If the subjects use a point of reference, the fact noted by M. Henri appears to be quite reasonable, and I think the cause is to be found in the fact that the image of the point of reference itself has a localizing motor element and this merged with the corresponding element from the sensation or image of the point stimulated is quite sufficient to account for the error in that direction.[2] The use of reflective judgment, however, in my experiments was eliminated, and my subjects apparently did not have in mind any point of reference of the kind which M. Henri describes. How then shall we account for this almost uniform tendency to displace in the direction of the hand, even when the stimulus was given near the elbow? It is quite possible, of course, that the hand is always used, even unconsciously, as a point of reference when the localization is made on the forearm, but I think there is reason to believe that another factor plays a more conspicuous part. It is well known

[1] The figures showing the average error, mean variation, etc., are given in centimeters.

[2] The analogy between such a 'point of reference' and the zero point or focus of attention at the beginning of the auditory and visual experiments is manifest. See pp. 343 and 350.

that the nerve terminations in the skin are more numerous as we pass downward toward the extremities. Further, it has often been noted that in stimulating the skin we do not stimulate merely a point, but, on account of the tension of the skin, we stimulate an area. Inasmuch, then, as the nerve supply in such an area is greater in the lower half, it is quite conceivable that the attention should be focused not on its mathematical center, where the point was stimulated, but on a center of equilibrium, which must, by reason of the nerve distribution, fall in the lower half. President Hall, in explaining similar facts brought out in some of his investigations,[1] finds their origin in the genetic development of the race. Erroneous estimations of the sensations of movement involved in the localizations have also been adduced as responsible for the tendencies manifested.

Referring again to Table A, we find that when the second stimulus is given above the first, the error downward is changed into an error upward, and that in the single case where the natural error was up, the error is considerably increased when the second stimulus is given. On the other hand, when the second stimulus is given below the first, the natural error downward is considerably increased in the direction of the second stimulus. Table A contains a summary of all of the experiments. The first series of experiments does not show the same results as the later ones. In the first experiments the disposition to resist the influence of the second stimulus, and even to make an error in the opposite direction, was quite manifest. These results being included in the averages shown in Table A, have the effect of decreasing the average value of the influence of the second stimulus. It was my first intention, therefore, to leave out of consideration the results of the first series, regarding them as mere control experiments, but the gradual yielding to the suggestion as it is repeated time after time appears to me now as one of the most significant facts developed during the course of the experiments. In order, therefore, to show the change in the form of reaction, I present Table B, which shows all of the series of reactions obtained from a single subject. It will be

[1] See Hall and Donaldson, *Mind*, Vol. X., p. 557; also Hall and Motora, *Amer. Jour. of Psychology*, Vol. I., p. 72.

observed that when the second stimulus[1] is given above the first, it begins to have a consistent positive influence after the second series, and that when the second stimulus is given below the first, a consistent positive influence is not manifest until the middle of the fourth series. This table is fairly typical of the results derived from each subject. The subject is a graduate student of philosophy, accustomed to critical introspective analysis. He was not cognizant of the purpose of the experiments, and when questioned at the end was, like all the others, ignorant of the effect produced by the application of the second stimulus.

TABLE B.—TACTUAL.—SUBJECT G.[2]

Series.	Region of Arm.	Single Stimulus.				Suggestion Up.								Suggestion Down.							
		Tests.	Av. Error.	Direction.	M. Variation.	Tests.	Av. Error.	Direction.	M. Variation.	Per cent. of Pos. Influence.	Per cent. of Neg. Influence.	Positive influence in cm.	Neg. Influence in cm.	Tests.	Av. Error.	Direction.	M. Variation.	Per cent. of Pos. Influence.	Per cent. of Neg. Influence.	Positive Influence in cm.	Neg. Influence in cm.
I	Mid-arm.	9	2.13	D	.92	9	.52	D	.80	89	11	1.61		9	.54	U	.44		100		2.67
	Wrist.	7	.20	U	.47	7	1.81	U	.41	100		1.61		7	2.20	U	.78		100		2.00
	Elbow.	9	1.82	D	.55	9	.06	D	.86	89	11	1.76		9	2.95	D	.65	89	11	1.13	
II	Mid-arm.	9	.33	U	.67	9	.03	U	.67	45	55		.30	9	.29	U	.33	45	55		.06
	Wrist.	8	.50	U	.20	8	2.60	U	.72	100		2.10		8	2.15	U	.67		100		1.65
	Elbow.	9	1.60	D	.87	8	.37	D	.37	75	25	1.23		8	3.61	D	.58	100		2.01	
III	Mid-arm.	10	.60	U	.30	10	3.01	U	.69	100		2.41		10	1.32	D	.66	100		1.92	
	Wrist.	9	1.03	D	.47	9	2.53	U	.90	100		1.50		16	.59	U	.62	7	93		1.62
	Elbow.	8	1.17	D	.52	9	3.43	U	.88	100		2.26		9	2.60	D	.42	100		1.43	
IV	Mid-arm.	7	1.17	D	.57	7	1.55	U	.71	100		2.72		9	1.10	D	.95	55	45		.07
	Wrist.	9	.88	D	.27	9	2.55	U	.77	100		3.43		8	.62	U	.63		100		1.50
	Elbow.	10	2.01	D	.67	10	1.54	U	.60	100		3.55		9	2.98	D	.72	90	10	.97	
V	Mid-arm.	8	.55	D	.37	9	1.98	U	.27	100		2.53		9	1.47	D	.53	78	22	.92	
	Wrist.	15	.56	U	.90	7	1.53	U	.60	86	14	.97		9	.31	U	.63	55	45	.05	
	Elbow.	8	.21	D	.46	9	1.44	U	.42	100		1.65		8	2.86	D	.43	100		2.65	
VI	Mid-arm.	8	1.15	D	.25	8	1.62	U	.52	100		2.77		8	2.03	D	.51	87	13	.88	
	Wrist.	10	.49	D	.39	10	2.00	U	.70	100		2.49		9	.63	D	.41	55	45	.14	
	Elbow.	9	1.22	D	.35	9	.55	U	.41	90		1.77		9	3.38	D	.70	100		2.16	

Variation in the intensity of the second stimulus affects the influence of the stimulus. Three different intensities were tried, —less, equal, and more intense than the first stimulus. I offer only a single illustration. When the second stimulus was less

[1] In the case of this subject, the two stimuli were given simultaneously throughout the experiment. Occasional observations have been made showing a tendency, when two such stimuli are felt as one point, to locate this midway between the two points actually touched. Henri's experiments do not confirm these reports. *Cf.* Henri, Raumwahrnehmungen d. Tastsinnes, p. 130.

[2] The average errors, mean variation, etc., are given in centimeters.

intense than the first, it produced a change in the reaction as compared with the normal of 1.51 cm.; when it was equal in intensity to the first, its effect was 2.17 cm.; and when more intense than the first, the effect was 3.00 cm. A change in the distance of the second stimulus from the first also affects the result. The greatest effect is produced when the second is distant from the first about 8 cm. in the case in which the second stimulus is given above the first. In the other case the greatest effect is produced at a distance of about 10 cm. Either increasing or diminishing this distance diminishes the influence of the second stimulus. The reason for the decrease in effect when the distance between the two points decreases is apparently simple and may be best explained according to the principle of the parallelogram of forces. But the other case is not so clear. I take it, however, that when the distance is greater than 10 cm., the sensory quality of the two stimuli becomes so very different, that attention is more definitely aroused and the compensatory tendencies, which were so manifest in the earlier experiments, are again more definitely engaged in the reaction. These tendencies are very manifest when one stimulus is given on the arm and the other on the palm of the hand—the sensory quality of the two regions being distinctly different. It appears quite probable, however, that even in these cases, if the experiments were continued long enough for the two qualities of sensation to become habitually associated, so that attention is no longer disturbed by the difference in quality, these compensatory tendencies would gradually disappear and we might thus finally say that the effect of the second stimulus is proportional to its distance from the first. The foregoing statement certainly agrees with, and possibly in a measure explains, the change in the direction of the error in the early stages of the experiments, to which reference has already been made.

Later Tactual Experiments.

For the purpose of comparing the results of the tactual with the auditory and visual experiments which are to follow, I have carried on another series of tactual experiments, in which I have endeavored to use a method similar to that which

I found it expedient to adopt in the case of the auditory and visual investigations. The subjects were two boys, aged respectively eight and seven years. They also served as subjects for the auditory and visual experiments, and hence the results from all three lines of experiments are comparable.

The method used was as follows. Beginning at a distance of 6.5 cm. from the junction of wrist and palm of hand, on the volar surface of the forearm, I marked off five lines parallel to each other, one centimeter apart, and perpendicular to the long axis of the arm. The stimulus to be localized was given on one of these lines, beginning with the one nearest the wrist and proceeding upward in regular order. Sufficient extra stimuli were introduced in irregular order to prevent the subject from forming too definite a conclusion as to the order. This precaution was observed in all the experiments. The same process as that just detailed was again repeated with the addition of a second stimulus given immediately after and 6 cm. below the first. The stimulus acted as a suggestion 'down.' The process was again repeated, but this time the second stimulus was given 6 cm. above the first and acted as a suggestion 'up.'

The accompanying table (C) represents three complete series for both subjects of ten tests each for each of the five standard points.

TABLE C.—TACTUAL.[1]

Series.	Subject.	Single Stimulus.				Suggestion Down.						Suggestion Up.						Per cent. of Suggestibility.	
		Tests.	Av. Error.	Direction.	M. Variation.	Tests.	Av. Error.	Direction.	M. Variation	Positive Influence in cm.	Neg. Influence in cm.	Tests.	Av. Error.	Direction.	M. Variation.	Positive Influence in cm.	Neg. Influence in cm.		
I	T. P.	50	.4	D	.8	50	.8	D	.8	.4		50	.7	U	.9	1.1		68	
II		50	.8	D	.9	50	1.0	D	.6	.2		50	.4	U	.6	1.2		84	
III		50	.7	D	.6	50	1.4	D	.6	.7		50	1.8	U	.8	2.5		82	78
I	H. P.	50	.9	U	.8	50	2.0	U	.6		1.1	50	2.4	U	2.0	1.5		51	
II		50	.7	U	.7	50	1.2	U	.9		.5	50	2.6	U	.8	1.9		61	
III		50	.6	U	.8	50	.2	U	.9	.4		50	1.6	U	.8	1.0		76	63

[1] The average errors, mean variations, etc., are given in centimeters.

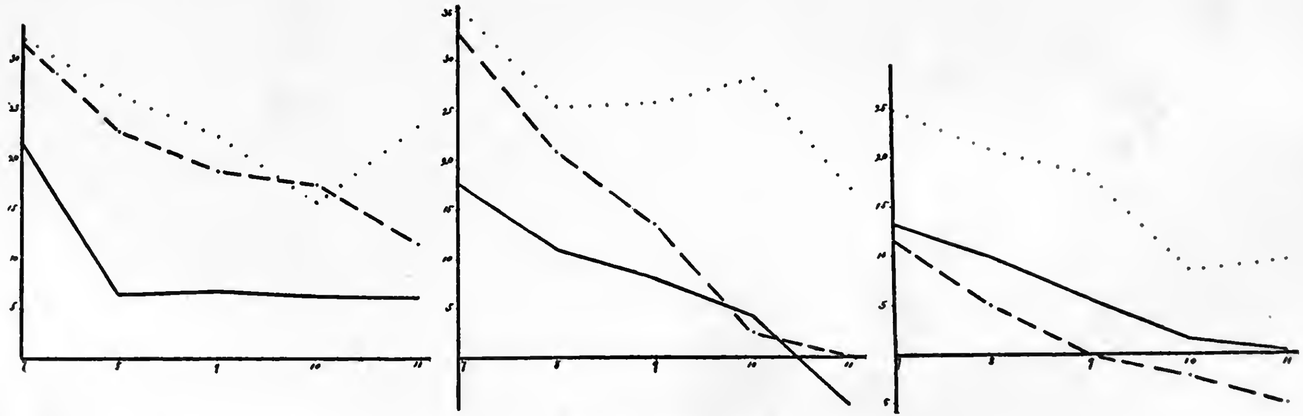
It will be noticed that the second subject, H. P., contradicts what I have previously said as to the direction of the normal error downward.[1] Again, in the first series, when suggestion 'down' is given, *i. e.*, a suggestion opposed to his normal tendency, he reacts against the suggestion in the opposite direction. This contradiction is decreased in the second series and the suggestion is fully effective in the third series. On the other hand, it is interesting to observe that the suggestion in the direction of his normal tendency, *i. e.*, up, decreases in force from the first to the third series. I shall have occasion to note and comment upon similar variations in the other experiments. The fact that such variations are constant under such diverse conditions would appear to have considerable significance.

In order to portray graphically the change in the character of the reactions, as well as the other features of the experiments, I have constructed curves representing each of the three series for the subject H. P. The points on the ordinate, viz. 7, 8, 9, 10, 11, represent the points on the forearm where the stimuli were given. These were respectively 7, 8, 9, 10 and 11 cm. distant from the juncture of wrist and palm. The figures on the abscissa represent the error in localization measured in millimeters. The general downward tendency of all of these curves is particularly remarkable, inasmuch as it shows that the absolute error in localization decreases as we proceed upward toward the elbow. This is probably due to a variation in the force of some motor element, which is not clearly apparent. There was no reason to suspect any peculiarity in the sensory discrimination for the region and this motor factor is seemingly the only direction in which an explanation could be found.

Summary of Results.

I. In the localization of single points stimulated on the forearm there is a general tendency to displace downward, *i. e.*, in the direction of the hand.

[1] I could discover nothing in the conditions of the experiment which would account for this contradiction. Outside of the possibility of the subject's using a different 'point of reference,' which I think improbable, I know of no way to account for the contradiction, except by reference to the general character of the subject.



MEAN VARIATION.

Subject H. P. (Tactual)	Fig. 1.	Fig. 2.	Fig. 3.
——— Single Stimulus,	7.7	7.2	8.0
- - - - Suggestion Down,	6.4	9.8	9.1
. . . . . Suggestion Up,	10.5	8.2	8.6
Average Suggestibility,	51%		76%

II. If a second stimulus is given *above* the first and the subject is required to localize the first stimulus, the normal error downward at the outset is *increased;* if the second stimulus is given *below* the first and the subject is required to localize the latter the normal error downward is at the outset *diminished.*

III. But, after a time, varying with each subject, the tendency just mentioned is reversed. A second stimulus below the first increases the normal tendency to displace downward and a second stimulus above the first changes the tendency to displace downward into a tendency to displace upward, *i. e.*, in the direction of the second stimulus. This second tendency is apparently permanent, so long as the stimulations are repeated. Whether reversion to the original tendency would ever occur cannot be said.

IV. The absolute effect of the second stimulus when given below is less than when given above the point to be localized. In other words, a suggestion to increase the normal tendency to error is less effective than a suggestion to reverse the nature of the error.[1]

V. The distraction of attention produced by the second stimulus is shown by a slight increase in the average variation. This increase is greater when the suggestion is 'up,' *i. e.*, in the direction contrary to natural displacements.

VI. The effect of the second stimulus is proportional to its intensity relative to the first.

VII. The greatest effect of the second stimulus results when it is given at a distance of eight to ten centimeters from the first.

Auditory.

The second series of experiments represents an effort to determine the influence of a sound, when the attempt is made to locate another immediately preceding sound of like character. The subjects used were eleven in number, seven men, two women and two boys. The two boys and three of the men also served for the visual experiments, thus affording some basis for comparison.

[1] This principle will be slightly modified in connection with the auditory and visual tests.

The apparatus employed in the experiments was constructed as follows: A semicircular table, six and a half feet in diameter and three and one-third feet high, bore on its outer circumference and extending above it to a height of six inches, sheets of white cardboard, on which were drawn the degrees of a circle. Back of this cardboard was a black curtain extending entirely around the outer circumference of the table and elevated two feet above it, thus concealing entirely the movements of the operator from the subject, as well as affording a homogeneous background. The subject is seated in the center of the circle, of which the table is a part, so that his eyes are equidistant from all points on the graduated cardboard, behind which the sounds are to be made. The sound, a short, sharp noise, is made with a metallic clicker commonly used in practicing the Morse telegraphic code. The eyes of the subject, at a given signal, are fixed upon a cross placed just above the zero point[1] in front of him and in the center of the scale. After two seconds a click is given at some point behind the graduated cardboard in the right hand quadrant. The subject immediately follows the impulse to turn his eyes in the direction of the sound heard and calls the number below the point upon which his eyes rest. In the first experiments I selected ten definite points at which the sounds were always given. Later, I found it more expedient to reduce the number to five. After a single stimulus was given at each of these points, the same series was repeated, but another click was made at the subject's left, usually thirty degrees from the first click.

With the first group of subjects used, I unfortunately adopted the plan of giving the distracting stimulus first, and the stimulus to be localized last. This was done for the purpose of reducing the variable error to the minimum. Under these conditions, suggestion to the right, after the first few tests, is always effective, but in the majority of cases the suggestion to the left, *i. e.*, toward the center, produces contradictory results—the localization is more accurate than with a single stimulus. The reason is plain. The attention is first carried to the distracting stimulus. But this point is nearer the point to be localized than

[1] See note on p. 335.

the zero point from which the attention should start. Hence the motor power of the distracting stimulus is *subtracted* from the motor power of the zero point, whereas, if the distracting stimulus is given after the stimulus to be localized, its motor power is *added* to the motor power of the zero point. With the second group of subjects, therefore, I adopted the latter method of procedure. The five points, which were to localized, were as follows: 30, 40, 50, 59 and 69 degrees. The sounds were usually given at these points in the order named, only such variation and introduction of extra stimuli being employed as were thought necessary to prevent the subject from being sure of the relative direction and distance of the succeeding sounds. None of the subjects were aware of the fact that constant points were selected, nor were they acquainted with the nature of the influence which the second stimulus was expected to exert. On being questioned at the close of the last series of experiments, the eleven subjects were somewhat divided in their opinion as to the effect of the second stimulus. One was aware of a tendency to follow the second sound; others thought it would probably have an opposite effect. But the majority thought that its only effect was to make the reaction somewhat less accurate. These impressions evidently resemble those reported in the tactual experiments.

Results.

Turning to the results themselves, I present Table D. This represents all of the tests with the second group of subjects, except a few made to determine the effect of variation as regards the distance of the stimuli apart, their relative intensity, etc. Reading through, for example, the first series of tests obtained from H., a man, we find that in 50 tests, when a single stimulus was given at the five points already named, he displaced the sound on the average 4.6 degrees to the left, *i. e.*, toward the center or zero point from which his eyes started. The mean variation for this series was 2.7 degrees. Again, in 50 tests, when a second stimulus was given 30 degrees to the left of the first, he displaced the first stimulus on the average 8.8 degrees to the left, showing that the second or suggesting stimulus has had a positive effect of 4.2 degrees on the average for each time

TABLE D.—AUDITORY.[1]

Series.	Subject.	Single Stimulus.				Suggestion to Left.					Suggestion to Right.					Per cent of Suggestibility.	
		Tests.	Av. Error.	Direction.	M. Variation.	Tests.	Av. Error.	Direction.	M. Variation.	Positive Influence.	Tests.	Av. Error.	Direction.	M. Variation.	Positive Influence.		
I	H	50	4.6	L	2.7	50	8.8	L	3.6	4.2	50	1.3	L	3.5	3.3	69	
II	H	50	3.4	L	3.2	50	8.5	L	3.3	5.1	50	2.8	L	4.9	.6	66	
III	H	50	14.1	L	2.9	50	16.0	L	3.1	1.9	50	.8	L	4.5	13.3	84	73
I	A	50	12.5	L	4.5	50	15.6	L	3.1	3.1	50	11.7	L	3.8	.8	70	
II	A	50	16.6	L	2.9	50	17.1	L	3.4	.5	50	6.9	L	4.2	9.7	72	
III	A	50	14.1	L	2.9	50	16.0	L	2.1	1.9	50	.8	L	4.5	13.3	84	75
I	M	50	5.2	L	6.6	50	8.3	L	3.0	3.1	50	3.3	L	4.5	1.9	69	
II	M	50	5.8	L	3.4	50	9.0	L	3.5	3.2	50	1.3	L	5.3	4.5	73	
III	M	50	6.2	L	3.9	50	10.1	L	3.9	3.9	50	2.0	L	3.0	3.2	86	76
I	T. P.	50	2.0	L	4.9	50	8.8	L	7.4	6.8	50	2.1	L	5.9	0	64	
II	T. P.	50	3.3	L	5.3	50	7.2	L	4.3	3.9	50	.3	R	6.8	3.6	72	
III	T. P.	50	2.6	L	4.9	50	5.5	L	5.9	2.9	50	2.8	R	6.9	5.4	62	66
I	H. P.	50	5.1	L	4.3	50	6.3	L	5.0	1.2	50	.9	L	6.1	4.2	63	
II	H. P.	50	3.0	L	6.5	50	6.8	L	6.2	3.8	50	1.1	L	6.4	1.9	64	
III	H. P.	50	4.3	L	3.1	50	7.8	L	5.1	3.5	50	2.2	R	7.3	6.5	72	66

it was given; mean variation 3.6 degrees. Similarly, in 50 tests, when the suggestion was to the right, there was a displacement of only 1.3 degrees to the left, showing a positive effect of 3.3 degrees for each suggestion, the mean variation with suggestion to right being 3.5 degrees. From this series I have calculated that H. showed a suggestibility of 69 per cent. This calculation was made in the following way: The normal error for a single stimulus at a particular point was determined by averaging the individual errors made at that point. This normal error was then compared with *each* error made at the same point under the influence of suggestion. If this suggestion is to the left and the error is now greater than the normal, it is considered that the suggestion has operated; if the suggestion is to the right and the error is now less than the normal, or if the usual error to the left has become an error to the right, *i. e.*, in the direction of the second stimulus, the suggestion is again consid-

[1] The figures showing average error, mean variation, etc., represent the degrees of a circle.

ered to have operated. Thus, comparing the number of times the suggestion was given with the number of times it proved effective, we secure an index of the suggestibility of the individual, at least for this particular kind of suggestion.

Turning now to the table (D) as a whole, we find that without exception all of the subjects, when a single stimulus is given, displace it toward the left, *i. e.*, in the direction of the center upon which the subject's eyes have been fixed. For the sake of comparison, I made a few experiments in the left quadrant and in this case the error is uniformly to the right—again toward the center. Binet, expressing a familiar view, has said that every image has a motor element, and Münsterberg and Campbell have made experiments, to which reference has already been made, showing, as they maintain, the 'motor power of ideas.' The fact just noted appears to sustain the theory of both of these contentions. It certainly appears insufficient to say that the inertia of the ocular apparatus is wholly responsible for this falling short in the localization of sound under the conditions described, though it is quite possible that this factor, together with the increased muscular effort necessary to turn the eyes toward the periphery, is partly responsible for the nature and the size of the error. It would appear, however, if Münsterberg and Campbell are right in attributing a certain motor power to the idea of the object from which the eye starts in its movement, that this motor power ought to be, under the same conditions, a constant quantity. At first glance the more detailed results of my experiments appear to contradict this theory. But I believe that this apparent contradiction is really a further confirmation. The detailed results show that the size of the error to the left increases with the distance from the center. This increase in the size of the error is not due to increase in the motor power of the original object of attention. But besides the increased muscular exertion made necessary by increased distance,[1] which might partially account for the change, there is also to be considered the increase in the number of ob-

[1] In a few exceptional cases the error decreased after the stimulus passed 45 degrees. This is possibly explained by the fact that the head was turned in that direction and the proportion of muscular effort necessary to turn the eyes was thus decreased.

jects intervening between the zero point and the point from which the sound arises. Each of these points, when imaged upon the retina, has its own motor power. This fact was well illustrated in a difficulty which I encountered with my apparatus as first constructed. The numbers were written by "fives" upon the graduated cardboard, and the line representing each fifth degree was slightly elevated above the intervening ones. I found that the subject almost invariably localized the sound at one of these prominent points and consequently I altered the apparatus, so that all the lines representing the degrees were equally prominent and were extended twelve inches upon the table and there numbered. Thus the subject was required first to select the line and then to trace it to the end, before he could determine the number which it represented.

Another interesting fact is revealed by examination of Table D. There is an evident tendency in the three series of tests for the positive influence of the suggestion, when given to the left, to decrease as the experiment proceeded from Series I. to III.; whereas, on the other hand, there is an equally evident tendency towards an increase in positive influence when the suggestion is made toward the right. When it is remembered that 'toward the left' is the normal tendency to error and that in connection with the tactual experiments I said that a suggestion opposed to a normal tendency was more effective than the reverse, the real significance of the fact just pointed out becomes more apparent. We shall later find the same fact manifest in the visual experiments.

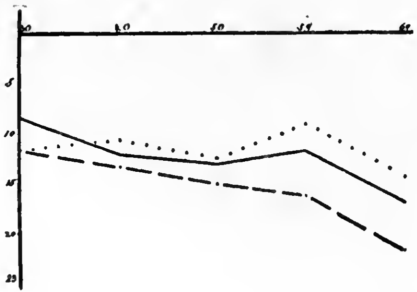
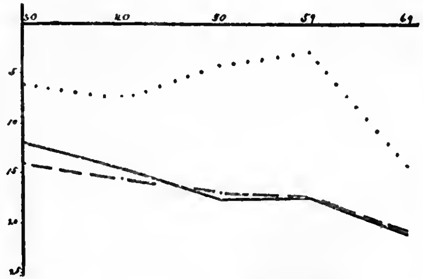
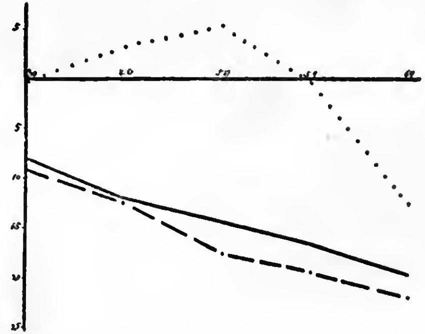
There is usually a slight increase in the mean variation when an interrupting suggestion is given, and as a rule the increase is greater when the suggestion is toward the right, *i. e.*, opposed to the normal tendency. All of the subjects except one show an increase in suggestibility from Series I. to III. The tendency to vacillation is sometimes very great at the first sitting and this tendency sometimes continued throughout an entire series of experiments, which usually occupied two or three days. The exception noted above is due doubtless to the conditions under which the third series of experiments was made. They were more hurried than usual and outside disturbances were more annoying.

In order to represent graphically the facts, which are brought out less clearly by the tabular statement just presented, I present some curves showing the progress of the experiments with a single subject, these curves being fairly typical of the results obtained from each of the subjects. In Figs. 4–6 are shown three curves. The points at which the stimuli were given are represented on the ordinate as 30, 40, 50, 59, 69, these figures representing degrees of a circle, counting from the center of the table, where the subject's eyes are fixed upon the zero point, to the right. The degrees of error in localization are represented on the abscissa as 5, 10, 15, 20, etc. The unbroken line represents the curve of normal error when a single stimulus is given; the broken line represents the curve of error when the suggestion is made toward the left; and the dotted line represents the error when the suggestion is made toward the right.

It will be noted that in the three series the normal error remains relatively about the same, the distance between the normal curve and the curve of suggestion to right increases. The general downward tendency of all of the curves is manifest.

Visual.

The problem here is to determine the effect of a second visual stimulus upon the localization of an object seen in peripheral vision. The object to be localized as well as the object used for suggestion was a piece of white paper 1 cm. square, pasted on a black cardboard 2 cm. square. These squares were attached to wooden rods, twelve inches long, which were arranged to slide noiselessly in grooves provided for the purpose, upon the outer circumference of the same table described in connection with the auditory experiments. The operator stood behind the screen and by pushing up one of the rods caused the visual stimulus to appear just above the edge of the semicircle. The stimulus appeared against the black cloth screen, which shielded the experimenter from the view of the subject. The points selected for giving the stimulus were 30, 40, 50, 59 and 69 degrees, the same as in the auditory experiments. After some preliminary experiments, 15 degrees was

MEAN VARIATION.

Subject A (Auditory).	Fig. 4.	Fig. 5.	Fig. 6.
——— Single Stimulus,	4.5 deg.	2.9 deg.	2.9 deg.
- - - - Suggestion to left,	3.1 "	3.4 "	3.1 "
. . . . . Suggestion to right,	3.8 "	4.2 "	4.5 "
Average Suggestibility,	70%	72%	84%

selected as the most convenient distance to maintain between the two stimuli. The stimulus to be located appeared first and the suggesting stimulus immediately thereafter. The conditions observed in the auditory experiments were maintained as nearly as possible in the visual tests. A square similar to that to be located was placed permanently above the cardboard at zero in the center of the table. At a given signal the subject fixated the square.[1] Two seconds thereafter a stimulus appeared at 30 degrees, continuing in the field of vision not more than two seconds. Immediately after its disappearance the subject followed the impulse to turn his eyes in the direction where the object appeared. He noted the line nearest the point where the object seemed to be, followed the line down upon the table and reported the number corresponding to it. After stimuli had thus appeared at each of the five points already named, the process was repeated in the same order with the addition of suggestions to the left, *i. e.*, immediately after the first stimulus was given at 30 degrees, a second stimulus appeared at 15 degrees, etc. The subject had been previously instructed not to move his eyes, until after the disappearance of the second stimulus. After suggestions to the left had been given in connection with each of the five fixed points, the same process was again repeated, save that the suggestions were now made to the right. Except with one subject the start was always made at 30 degrees and the series progressed regularly to 69 degrees outward. In the case of the exception just noted, suggestion to the right, under these conditions, had the effect contradictory to that usually produced in the case of the other subjects. But if, after reaching 69 degrees when giving suggestion to the left, I reversed the order and gave suggestion to the right, working from 69 degrees to 30 degrees, the results compared very favorably with those derived from the other subjects. I can attribute this result only to individual peculiarity. If translated into terms of ordinary life, it would seem to indicate that the subject is almost abnormally opposed to innovations, that it is with great difficulty he is influenced to alter his habits, etc. I questioned him concerning these matters at

[1] See note on p. 335.

the close of the experiments, and his account of his feeling with reference to such things agrees exactly with such a presupposition. It was interesting to observe, however, that even in the case of this subject, before the end of the experiments, the influence of the suggestion to the right, *i. e.*, opposed to his normal tendency, became much greater than the influence of suggestion to the left, even showing some abnormality here again. Thus we see that though a suggestion opposed to the normal tendency may be less effective in the beginning than a suggestion which agrees with a normal tendency, ultimately the principle which I have already enunciated obtains, even in such abnormal cases.

Results.

To show the results of all the visual experiments at a glance, I present Table E. The subjects are the same as those used in the auditory experiments, the conditions were the same as nearly

TABLE E.—VISUAL.[1]

Series.	Subject.	Single Stimulus.				Suggestion to Left.						Suggestion to Right.						Per cent of Suggestibility.	
		Tests.	Av. Error.	Direction.	M. Variation.	Tests.	Av. Error.	Direction.	M. Variation.	Positive Influence in cm.	Neg. Influence in cm.	Tests.	Av. Error.	Direction.	M. Variation.	Positive Influence in cm.	Neg. Influence in cm.		
I	H.	50	.6	L	1.3	50	3.0	L	1.5	2.4		50	0		1.6	.6		61	
II		50	.4	L	1.4	50	1.6	L	1.1	1.2		50	.8	R	1.3	1.2		72	
III		50	.5	R	.8	50	.2	L	.7	.7		50	.3	R	1.2		.2	54	62
I	A.	50	4.0	L	2.2	50	5.6	L	1.5	1.6		50	6.5	L	1.7		2.5	47	
II		50	5.0	L	1.3	50	6.5	L	1.2	1.5		50	5.3	L	2.4		.3	63	
III		50	4.5	L	1.1	50	5.2	L	1.0	.7		50	1.7	L	2.4	2.8		71	
IV		50	3.8	L	.9	50	1.6	L	1.1	.8		50	1.6	L	1.8	2.2		70	63
I	M.	50	5.2	L	1.3	50	7.4	L	1.4	2.2		50	5.0	L	1.5	.2		66	
II		50	5.1	L	2.0	50	6.1	L	1.7	1.0		50	3.8	L	2.3	1.3		69	
III		50	4.5	L	1.6	50	5.1	L	2.0	.6		50	2.6	L	2.6	3.9		65	67
I	T. P.	100	2.5	L	2.0	100	5.8	L	2.9	3.3		100	.9	L	2.8	1.5		80	
II		50	2.5	L	1.9	50	4.7	L	1.9	2.2		50	1.2	R	2.6	3.7		79	
III		50	.7	L	1.9	50	3.6	L	2.3	2.9		50	.9	R	2.3	1.6		76	78
I	H. P.	100	6.0	L	1.7	100	6.6	L	2.6	.6		100	4.6	L	2.0	1.4		54	
II		50	2.9	L	1.5	50	3.2	L	1.7	.3		50	1.7	L	1.4	1.2		51	
III		50	2.3	L	1.5	50	.9	L	3.3		1.4	50	.4	L	2.5	1.9		49	55

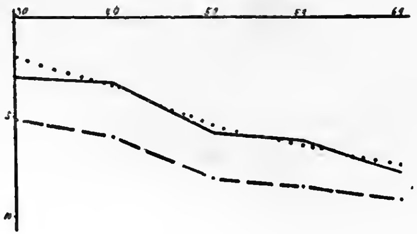
[1] Figures showing error, mean variation, etc., represent degrees of a circle.

as possible, and the results are of a remarkably similar character. As might be expected, the absolute error is less in the case of the visual tests, as is also the mean variation. The percentage of suggestibility is less in the case of the visual than in the case of the auditory series. This may be accounted for partly by the fact that the second stimulus in the visual experiments was only 15 degrees distant from the first, while in the case of the auditory experiments the distance between stimuli was 30 degrees. Taking into consideration the fact, which was established, viz., that the effect of the second stimulus varies with its distance from the first and also with its relative intensity, the difference in the percentage of suggestibility is not at all remarkable. It will be remembered that the absolute distances selected for the two groups of experiments were determined with reference to avoiding large variations in the localizations.

Showing the results of the visual experiments in a more graphic manner, I present some curves constructed similarly to those already shown for the auditory experiments. Figure 7 represents the curve of suggestibility of subject M., a man, as indicated by the first ten tests for each of the five standard points. Figures 8 and 9 represent curves of suggestibility for the same subject as indicated by the second and third series of ten tests each, for each of the five standard points. In comparing the auditory and visual curves it will be noticed that the scale on which they are constructed has been altered, the errors being somewhat exaggerated in the case of the visual.

Referring to the curves we see again the increase in error toward the periphery, shown by the gradual slope downward of all the curves; again we notice a gradual diminution in the power of suggestion, when given in the direction of the normal tendency; while the corresponding increase in power, when the suggestion is opposed to the normal tendency, is equally manifest.

The first effect of any change in the method of the experiments, *e.g.*, a change in the distance between the first and second stimulus, is to produce contradictory results. Attention is aroused and the subject reacts against the suggestion. I found it difficult, therefore, to get trustworthy results showing the effect of the change in distance between the two stimuli. To



MEAN VARIATION.

Subject M (Visual).	Fig. 7.	Fig. 8.	Fig. 9.
——— Single stimulus,	1.3	2.0	1.6
- - - - Suggestion to left,	1.4	1.7	2.0
. . . . . Suggestion to right,	1.5	2.3	2.6
Average suggestibility,	66%	69%	65%

get perfectly reliable results it would be necessary to perform the same number of experiments for each of the distances selected, and, perhaps, to have experiments at all the distances selected at each sitting. This would have hindered the chief purpose of my experiments and would have involved more time than I had at my disposal. However, from a limited number of experiments, I think I am safe in saying that a change in the distance between the stimuli results in a corresponding change in the amount of error. For example, with one subject, I found that in fifty tests, with the two stimuli 20 degrees apart, the total effect of the second stimulus amounted to 16.8 degrees; while, in the same number of tests, with a distance of 15 degrees between stimuli, the total effect of the second stimulus amounted to 14.9 degrees. There would obviously be upper and lower limits for the operation of such a principle.

Summary of all Results.

1. In localizing a single stimulus, whether tactual, auditory or visual, an error is ordinarily made in the direction of the point upon which attention is directed when the stimulus is given. This error notably increases with the distance of the stimulus from the point of attention.

2. When a second stimulus of like character to the first is given, there is at first manifest a tendency to resist the suggestion thereby offered. But this tendency is diminished as the suggestion is repeated, and ultimately the suggestion becomes fully effective, causing the subject to err in the direction of the second stimulus.

3. The resistance, or the contradictory tendency, is most vigorous when the suggestion is made in a direction opposed to the normal tendency to error;[1] but ultimately a suggestion opposed to a normal tendency is more effective than in the reverse case.

4. Variation of intensity and distance of second stimulus, within certain limits, is followed by a corresponding variation in effect produced. The first effect, however, of any change in the method is to produce vacillating and contradictory results.

[1] This is true certainly in the auditory and visual experiments; less certainly in the case of the tactual.

It would evidently be difficult to hit upon a wholly satisfactory basis for comparing the suggestibility manifested by the three tracts of the nervous system investigated in the present experiments. Adopting the rough working principle of percentages employed in this paper, it appears that there is no exact correspondence among the several forms of tests in the case of any of the persons who served as subjects. Nevertheless the correspondence is close enough to be decidedly significant of considerable homogeneity in this respect throughout the nervous system. More notable, perhaps, is the fact that the same person shows the highest degree of suggestibility in each form of the experiment. Furthermore, if we compare the percentages of any two individuals in the different forms of the test, we shall find the ratios remarkably similar. The single exception to this statement occurs in the case of one of the subjects (T. P.) whose hearing was defective and whose results in the auditory series cannot fairly be used. For example, the percentages of one subject were for the visual tests 55 and for the auditory 66. For another subject the figures are 62 and 73. The proportion thus becomes 62:73::55:66. This gives us 4,092 = 4,015. The proportions are not all as nearly exact as this one, but the principle involved seems to hold throughout. So far, therefore, as the present observations permit a conclusion, they serve to indicate that conflicting stimuli provocative of localizing movements possess relatively equal values, whatever portions of the human nervous system they may effect. Given the degree of suggestibility of two individuals for one of the three tracts of the nervous system, we may reasonably infer relatively equal degrees of suggestibility for the other two tracts. This, in any event, is the obvious implication of my results, subject always to such modification as specific circumstances may involve.

Such a criterion of natural suggestibility seems to possess considerable advantage in point of exactness over that used by Binet in his tests upon school children.[1] He investigated the amount of modification he could introduce by suggestive questions into the judgments the children made of the lengths of visual lines. Such a method, while giving interesting indica-

[1] Binet, 'La suggestibilité,' p. 20.

tions of the general differences among children of different ages and education, clearly involves many shifting conditions of a purely social kind, which jeopardizes seriously the definiteness of the results. My method is not applicable immediately to large groups. But the results of it are certainly free from some important sources of error incidental to Binet's method. It is in this connection not without interest, perhaps, that, in the case of the two young boys who served for me, my results confirm very strikingly the opinion of all who know them well, as regards their relative sensitiveness to suggestion. In the present status of the subject we must frankly admit that any criterion is wholly tentative. I venture, however, to call attention to the fact that the parallelism is more than hypothetical between the conditions of my experiments and the conditions of an individual suddenly immersed in a new social and moral environment. Both are submitted to stimulations of whose exact purport they are often quite unaware. The first reaction is one of withdrawal and aversion. But under repeated exposure to the stimulus, without any explicit consciousness of what is occurring, they gradually yield and take on a new habit, which is the expression of a process that has hardly at any point involved reflective consciousness.[1]

If the theories of hypnotic suggestion, which have been couched in terms of inhibition, can give a really satisfactory account of the suppression of inhibitive associative processes, such facts as are reported in this paper would seem to show that the nervous system is entirely capable of executing the remaining activities, which have to be accounted for in explaining the effectiveness of such suggestion. This matter has been sufficiently discussed, as regards the psychophysical mechanism concerned, in the body of the paper and need not be again subjected to comment. The motor impulses intrinsic, in adult life at least, to sensations and images contain the clue to adequate explanation.

[1] In connection with reflective consciousness, attention may be directed to the process of the judgment involved in such experiments as these. In cases of this kind there is essentially a balancing of motor tendencies within the nervous organism and the resultant of these tendencies *is* the judgment. As a concrete instance of one form of relationship between judgments and motor activities it is not altogether devoid of interest.

FROM THE WELLESLEY COLLEGE PSYCHOLOGICAL LABORATORY.

The Perception of Sound Direction as a Conscious Process.

BY E. A. McC. GAMBLE.

This report deals with an attempt to analyze the auditory perception of direction. The results have led rather to the discounting than to the high appreciation of timbre, pitch, and intensity as conscious criteria of position.

Preliminary Discussion.

Let us assume, for the moment, that (1) timbre, pitch, and intensity, (2) reflex and semi-reflex movements of the head and eyes in the direction of a sound, and (3) cutaneous impressions about the ears, neck or scalp all act from time to time as factors in the localization-consciousness. If they do so act, they are factors of a very different kind. The consciousness of timbre, intensity, or pitch must serve as the link or pivot in an ordinary case of associative supplementing. Such localization finds its parallel in the gauging of distance by visual size or distinctness of outline. On the other hand, definite reflex movements in response to a sound must be genuine motor local signs. They find their parallel in the reflex jerk of hand or foot toward any irritated bit of skin. Timbre, intensity, and pitch are homogeneous criteria,[1] that is, they are position marks in the character of the sound itself, but they are not in themselves place-ideas. The after-consciousness of a reflex movement is a heterogeneous criterion, yet it is in itself a crude localization, a rudimentary tactual-motor idea of direction. Finally, if sounds are localized by 'touches' due to air-impact, auditory localization is, *ipso facto*, reduced to cutaneous.

[1] Titchener, 'Experimental Psychology,' Instructor's Manual, Qualitative, p. 357.

As a matter of fact, all primary criteria for localization are exceedingly elusive. Certain painstaking and trained observers maintain that a telephone-click correctly localized at different points does not seem to them to differ in timbre or in intensity or in any other way. Undifferentiated sounds seem to them to be instantly succeeded by differentiated ideas of place. Moreover, most observers note very rarely that they either move or 'want to move' when they hear the familiar sound of an experimental series. While practice diminishes the 'startling' effect of a sound, it notoriously increases the accuracy of localization. Apparently, just as a tap on the forehead may seem instantly to call up a picture of one's forehead or the word 'forehead' without any perception or memory of movement, so a telephone-click above one's head may evoke an image of the top of one's head and of the telephone held above it without any consciousness of head or of eye-movement.

Motor local signs doubtless play an all-important part in cutaneous localization in the days when the baby so assiduously explores with hands, feet and tongue the surface of his own small person. Yet we habitually localize that which touches us without attending to any group of sensations due to movement and perhaps without being conscious of any. It is hard to tell whether the motor local sign disappears from the content which introspection grasps through inattention and forgetfulness, as the act becomes habitual, or whether it ceases to come into consciousness at all. At any rate, it is conceivable, on the analogy of cutaneous localization, that motor local signs should for a time play an essential part in auditory localization and should then lapse from clear consciousness.

A serious objection may, however, be urged against this supposition. Auditory localization is decidedly inaccurate and is obviously improved by practice. Now, in so primitive and fumbling a process, motor local signs might be expected still to appear. The only answer to this objection is that the introspection of any process of localization is extraordinarily difficult, and that when practice has made the after-examination of auditory localizations easier, the motor sign has had time to lapse. The difficulty in introspection seems to be due to the fact that local-

ization, whether correct or incorrect, definite or indefinite, is so exceedingly rapid. The adult, when wide awake, does not leave for an instant any impression entirely unlocalized, that is, without any spatial associations. The explanation of this greed for spatial connection is presumably teleological. At any rate, even if a sound is assigned to a wide and vague area or is 'heard in two places at once,' some place-idea usually flashes into consciousness before one can catch it in the kindling.

We may, perhaps, assume that an observer with some training in psychological experiments will know, if in the long run he gauges position largely by timbre or intensity, or by 'involuntary' movement, or by cutaneous impressions. It cannot be forgotten, however, that an observer always tends to call a process which he himself cannot analyze 'elementary' or 'intuitive' or something of the sort.

In this report, the introspection of six subjects of some general training is taken in connection with the degree and the kind of error in their results. Apart from the aid of introspection, light upon the process of auditory localization has been sought in the effect of suggestion on the degree and direction of error. Laboratory exigencies compelled the employment of many untrained observers in these latter experiments and the massing of their results. The greater the effect of suggestion, the more likely is auditory localization to be an associative process, and the less likely is it to be the outgrowth of inevitable reflex movement. This we may surely assume on the basis of our general knowledge of the effect of suggestion.

A sharp distinction must be drawn between (1) the primary criteria of position, such as timbre, intensity, a 'touch' on the ear or the like, (2) the terms of localization, that is, the mental imagery of place or direction, and (3) the way in which the observer indicates position to the experimenter. However the direction of a sound is revealed to the observer, he may image the place or direction visually or in tactile terms or may name it. However he thinks of it in the first instance, he may either name it or point it out to the experimenter. We certainly tend to move our hands when we turn our heads, and in the same direction. Willing and ready pointing may, therefore, be an

indication of tactual-motor place-imagery and of a tendency to reflex movement of the head. The indication is, however, very insecure. With one of the six observers mentioned, ready pointing resulted simply from unusually definite visual imagery.

Apparatus and Method.

The sounds were all given at points on the surface of an imaginary sphere, having a radius of one half-meter and centering in the middle of the line connecting the observer's drum-membranes. The cardinal points were R and L, directly to the right and left in the 'auditory axis,' F and B, the points directly in front and behind in the same plane, U, the point overhead, 90° from the horizontal plane, and theoretically, D, the point 180° from U and under the observer's chair. The arcs limited by these points are called RF, LF, FU, and so on. Points on the horizontal and sagittal meridians are indicated by counting the number of degrees from R or L; points on the median meridian are counted from F or B. Thus, FU30 means 30° up from F on the median meridian. Directions are indicated in terms of opposite cardinal points. Thus, FB means backward, BF, forward, and so on.

In all the experiments, the sound was a telephone-click of fairly uniform intensity. The circuit was made and broken with a push-button two seconds after the spoken word 'Ready!' In the earlier experiments, the original Pierce and Münsterberg apparatus[1] was used with unimportant modifications. In the later experiments, the Titchener 'sound-cage' was employed. As adapted to our purposes, this apparatus consists of (1) an iron gas-pipe fixed in the ceiling, (2) a semi-circle of heavy brass wire, 102 cm. in diameter, rotating freely about the pipe as a vertical axis, and (3) a similar semi-circle just fitting within the larger, suspended from it, and rotating freely about a horizontal axis.[2] The vertical semi-circle is braced with a metal cross-bar. The telephone receiver is clamped to the center of the smaller semi-circle. Each semi-circle is provided

[1] Psychological Review, Vol. I., p. 464.

[2] Cf. Titchener, 'Experimental Psychology,' Student's Manual, Qualitative, p. 179. The apparatus is made by the Chicago Laboratory Supply and Scale Co.

with a disc graduated to hundredths and with a pointer. The readings give the position of the telephone in terms of the 'latitude and longitude' of the imaginary experimental sphere. The sound can be given exactly at any point except within a segment of about 72° in diameter which is cut out of the lower hemisphere by the chair and person of the observer. Moreover, one can instantly find the direction and degree of error if one moves the telephone to the point at which the sound is localized by the observer and takes the new readings. The apparatus is noiseless.

Of the six observers who came repeatedly and whose introspection has been especially noted, Cs. is an instructor in psychology, Ck. was laboratory assistant at the time the experiments were made upon her, D., P. and S. were senior students in a second year course in psychology, and G. is the writer.

The experiments fall into four groups: (1) Experiments with and without suggestion made upon unpracticed observers with the Harvard apparatus; (2) experiments of the same kind with the Titchener apparatus; (3) experiments on the observers, Cs., D. and G. with the Harvard apparatus; and (4) experiments on Ck., P., S. and G. with the Titchener 'cage.'

The details of method which are really important are : (1) the attempt to eliminate all anticipations of position, and (2) the pointing required of the six special observers. With the Pierce and Münsterberg apparatus, the range of positions from which the experimental series were made out included sixty-nine; with the Titchener apparatus it included one hundred and sixty-one. No observer had the slightest intimation of the points to be selected from these wide ranges. Only G. exactly knew the range of points from which the series were drawn. The earlier observers saw the Harvard apparatus. With the Titchener apparatus, however, the subjects were blindfolded,[1] and no one of them, except Ck. and G., had ever seen it. Under these conditions, it was necessary for all the observers except G. to indicate most positions by pointing. One complete series was

[1] The bandage was of black China silk, loosely folded ; it extended merely to the temples, and was secured by narrow ribbons. It surely could not modify the sound impression.

taken with G., in which she simply pointed in the direction of the sound and avoided words, and another in which she stated position in terms of the apparatus readings in the series, all multiples of five. The other observers were allowed to correct their pointing in words. With the Harvard apparatus, unpracticed observers were permitted to indicate position in any way they pleased. Cs., D. and G., however, were required to point out the position of the sound first with the eyes shut and then with the eyes open.

These two circumstances taken together, (1) the elimination of expectation by the use of so many positions, and (2) the pointing, sufficiently account for the small number of right cases shown in the tables. They seem absurdly small as compared with the results of other experimenters. With an approximation to the conditions of Professor Angell and Dr. Fite and of Mr. Matsumato, however, our results approximate to theirs. Naturally, our observers often said that the sound came from a point far within the surface of the sphere. In these cases, the outer termination of the radius passing through the point (as nearly as we could guess at the line) was taken as the localization point. A given trial might not be repeated unless the subject professed inattention or complained of some indistinctness in the sound. This rule also tended to diminish the number of right cases. It was sometimes broken, however.

The questions put to the observers were: (1) How do you know where the sound is? (2) Do you seem to see the place or do you feel yourself move toward it or do you think it in words? Other questions were dropped as unduly suggestive.

Introspective Results.

Statements in regard to the terms of localization will first be considered, and second, testimony as to the primary criteria, the conscious clues to the place.

With G., visual imagery has been present from first to last in every series of experiments. The visual idea of place is deliberately translated into tactile terms, just as one pictures the relative position of objects in a dark room and then 'feels about' for them accordingly. Cs. and D. reported visual imag-

ery in nearly every case for which they gave any introspection. Cs.'s remarks on the required pointing show that it was secondary and deliberate. D.'s introspection is very scanty. No comment on the act of pointing can be found. Cs. and D. pictured some part of the 'cage' more often than G. did; G. 'saw' her own person, in the form of a vaguely outlined shadow, more often than Cs. and D. visualized themselves. Cs. and D. often pictured the cage without the telephone; G. almost invariably saw in a given position the telephone or a spot or streak of luminous color standing for the sound.[1]

Ck., on the other hand, showed a clear case of tactual-motor localization. She 'could have a clear picture of the room and the telephone if she wanted to, but the hearing of the sound did not call up the picture.' She 'simply heard and pointed, deliberately and not on impulse.' To this testimony she consistently adhered. P. and S. could give no lucid account of their imagery. At first, P. certainly had visual imagery, for she spoke repeatedly of a 'black marble' or 'ball of polished oak.' S. (although she also is a 'color hearer') seems to have localized rather more largely in words than did the other observers.

The great majority of the wholly unpracticed subjects saw something near their own persons—a line of some kind leading to their heads, a wooden ball, an ebony box, or the like. Two blind observers were tested. H. is a young woman who lost her sight by an accident at eight years old, and is now a teacher of the blind. She said that she 'saw the place in her mind' and that 'pointing was an expression of what was in her mind.' R. is a student at the Perkins Institute and has been blind from birth. She could give no clear account of her experiences. She said, however, that when she heard the sound, she 'pointed at once so as not to lose it.' Pointing seems, therefore, to have been deliberate.

[1] This observer shows a marked case of colored hearing. The color seen at first and most often was a sort of corn-color, the color of the click. Sometimes, however, pink, brown, white, green, or golden yellow would appear. The colors could not be anticipated. Of late, no color has been seen except the pale dull yellow of the sound. These phenomena are of interest in showing the highly visual character of the localization.

To turn to the primary criteria of localization. Cs. spoke in two cases out of 209 of a difference in intensity and once of a difference in timbre. D. spoke in six cases out of 248 of a difference in intensity; never, of a difference in timbre. G. knows that sounds given directly above her head are fainter with the Titchener apparatus than other sounds. It took her some time to discover this fact, but of late she has been able to use it successfully as a criterion. It must be noted that it was gleaned from the observation of sounds already localized with assurance. She has sometimes fancied that sounds given behind are 'thin' but makes mistakes in using this thinness (a visual character) as a criterion. Ck. noted no such criteria. P. said, late in the course of the experiments, that 'something in the sound' told her whether it was 'up or down,' 'right or left,' but not whether it was 'back or front.' She frankly remarked, however, that she thought she had 'once heard something of this sort in class.' S. said that sounds given in front and above were 'more explosive.' H. said that the sounds had an 'individuality due to their pitch' (timbre?). An unpracticed observer would occasionally allude to a difference in intensity or 'pitch.'

On the other hand, Cs. rather often (in fully 50 per cent. of her total number of cases) spoke of a touch, or a tickling or tingling somewhere on the scalp or on the inside of the ear. These pressure experiences often preceded her visualizations. The sound, however, was not always located on the same side as the skin impression. G. occasionally noted 'touches' on the head. She certainly feels the vibration when the sounds are given very far down and, therefore, very close to her body. D., Ck., S., H. and R. never noted such experiences. Other subjects, however, noted them not infrequently. Stress is laid on these experiences since Professor Angell and Dr. Fite say that, in their monaural experiments, they 'found no good evidence for supposing that cutaneous sensations played any part in the localizations.'[1]

Farther, Cs., G., D. and Ck. sometimes noted a tendency to involuntary head-movement. Ck. maintained that she had a general tendency to move her head toward the sound. G. did

[1] PSYCHOLOGICAL REVIEW, Vol. VIII., p. 246 (May, 1901).

actually move more than other observers, but usually did so quite unconsciously. Cs. noted a feeling of movement in the greatest number of specific instances—in only about 6 per cent. of her total number of cases, however. The only testimony which indicates automatic hand movement is P.'s. She said, "If the stimulus should act more slowly or by steps, perhaps I could introspect, but it all happens so fast. I hear the click and realize that I know where it is. At about the same instant my finger is at the place I hear it."

To summarize: The only observer explicitly to claim a primary criterion was Ck., a tactual-motor localizer. She maintained, as a general observation, that head-movement was 'the natural way to localize a sound,' but that she pointed to the place deliberately. On the other hand, P., who sometimes visualized and who noted differences in the quality of the sound in different positions, made automatic hand-movements, independent of the quality-feeling. Cs. imaged position in visual terms, but the visualization was often preceded or accompanied by cutaneous impressions and sometimes by a consciousness of head-movement. Of D. we know only that she visualized constantly and that she once or twice noticed involuntary head-movements. S. localized in part verbally. She insisted that she 'could not tell how she knew where the sound was.' G. has always visualized sounds. She explicitly maintains that there is ordinarily nothing in consciousness between the 'sight' of the sound and the developed picture of a spot of color or of the telephone in a certain position. One passes into the other as if figures about a central figure, already dimly seen, came swiftly through a mist. She has developed one intensity criterion by experience, and uses it when the sound looks like a streak through the whole median plane. She occasionally is guided by 'touches' and sometimes perhaps by head-movements.

We may anticipate in saying that P., G. and Ck. localized with comparative accuracy; that Cs. and D. in spite of practice localized with about the average accuracy of unpracticed observers; and that S. and, strange to say, the two blind observers, H. and R., fell far below this average.

Experimental Results.[1]

Figures will be given simply (1) to show one peculiarity which runs through all of our results, (2) to make clear some of the individual differences of the observers who were studied separately, and (3) to indicate the effect of suggestion upon the ordinary tendencies in localization.

In the first place, it would seem from our results that observers, at least in the beginning, tend to localize sounds given at B correctly and to localize all other sounds farther back than they are given.[2] From 32 unpracticed observers tested with the Titchener apparatus, the following massed results were obtained:

Table I.

SHOWING THE TENDENCY OF UNPRACTICED OBSERVERS TOWARD LOCALIZATION IN THE REAR.

Point.	Cases.	Right Cases per cent.	Direction of Error. Cases in per cents.			
			FB.		BF.	
			Under 72°.	Over 72°.	Under 72°.	Over 72°.
F	117	2	14	81		
FU 36	119	1	9	80		
FD 36	119	1	3	94		
B	120	24			57	3
BU 36	118	10			59	4
BD 36	118	14			54	1
U	52	12	37	52		
R	103	25	38		28	
RU 36	118	5	36	1	45	
RD 36	117	11	41		40	
L	104	22	49		15	
LU 36	118	8	43		34	
LD 36	119	7	45		31	
Total,	1442	11	23	23	29	1

The number of small errors in localizing points to the rear is undoubtedly increased by the difficulty of pointing backward with precision.

The backward tendency is quite as striking in the results of

[1] The following students served much as experimenters: Misses A. P. Cromack and C. M. Locke, 1900; D. Donner, 1901; H. B. Decker and M. B. Wood, 1902, and E. E. Pennell, M.A. 1901. Many other students took part as experimenters. Special acknowledgment is due to Miss M. C. Smith, laboratory assistant, for help in the later work.

[2] The same tendency appears in the Angell and Fite results, though not in so marked a degree.

the subjects tested with the Pierce and Münsterberg apparatus. Points are correctly localized at B and BU 30 more than twice as many times as at F and FU 30. Localization is more accurate in the whole of the backward hemisphere. The points most correctly localized are not due R and L, but RB 60 and LB 60. The following results (including only errors which could be counted in degrees upon one meridian) were obtained from about 60 unpracticed subjects:

Table II.

ALSO SHOWING THE TENDENCY OF UNPRACTICED SUBJECTS TOWARD LOCALIZATION IN THE REAR.

Meridian.	Cases.	Direction of Error.	Degree of Error. Cases in per cents. Under					
			30°.	60°.	90°.	120°.	150°.	180°.
Median.	733	FB	10	15	9	2	4	4
		BF	8	7	2	.1	.7	.5
Right horizontal.	729	FB	8	2	2	2	.7	
		BF	8	.7	.3	.1		.1
Left horizontal.	732	FB	7	2	1	.8		
		BF	6	2	.3		.1	

The majority of errors which could not be computed upon one meridian consisted in taking the arcs RF and LF for RU and LU, an illustration of the same backward tendency.

Table III.

SHOWING THE INDIVIDUAL DIFFERENCES OF THE SIX SPECIAL OBSERVERS.

Observers. Münsterberg Apparatus.	Cases.	Right Cases per cent.	Direction of Error.
Cs., eyes open.	205	9	Tendency : FB : BF :: 27 : 26
Cs., eyes shut.	209	12	FB : BF :: 24 : 30
D., eyes open.	248	11	FB : BF :: 19 : 27
D., eyes shut.	198	10	FB : BF :: 18 : 29
G., eyes open.	225	24	FB : BF :: 23 : 30
G., eyes shut.	188	24	FB : BF :: 21 : 27

Titchener Apparatus.	Cases.	Right Cases per cent.	Cases in per cents. FB. Under 72°.	FB. Over 72°.	BF. Under 72°.	BF. Over 72°.
Ck. Pointing blindfolded.	441	18	24	13	24	3
P. Pointing blindfolded.	448	22	31	5	19	7
S. Pointing blindfolded.	426	6	39	23	19	.9
G. Pointing blindfolded.	450	18	28	5	23	17
G. guessing at apparatus readings.	421	26	24	4	15	8

Table III., page 367, shows the comparative accuracy of the six special observers, and also their relative tendency to 'backward errors'.

The 'tendencies' FB and BF in the first part of the table are estimated by taking the percentage of errors FB and BF in relation to the number of cases given in front of the sagittal plane and behind it. With the Titchener apparatus, the points at which the sound was given were evenly divided between the hemispheres.

From these figures it would seem that the tendency to localize behind is not coupled with strong visualizing tendencies. From the fact that the tendency is so strong in the massed results of 90 unpracticed subjects it would seem that it is incident to the first systematic attempts at localizing sounds. It may be that the tendency has some teleological significance. The ears of an animal in a measure guard his rear. The tendency to turn vanishes as a sound becomes familiar. This is the only explanation which can here be offered. At any rate, the backward tendency is not due in any way to the position of the apparatus or of the experimenter. The apparatus was set up in two different rooms, and the direction in which the observers faced was, for a time, systematically altered to exclude constant sources of error.

To turn in the second place to the peculiarities of the six special observers: Cs., D., Ck., P., S. and G. It is hard to tell whether P. or G. is the more accurate localizer. G. is a very eye-minded observer, and is thus perhaps at a disadvantage in blindfolded pointing as compared with P. and Ck. It would seem from the first part of Table III. that a visualizing subject may be quite as accurate in pointing with closed eyes as in pointing with open eyes, provided he opens his eyes every moment or two and so gets his bearings. In the dark, however, the visualizer soon 'loses his place,' and his hand-movements resemble the random padding about of a baby who has not learned to 'use his hands.'

At any rate, the three most accurate observers all claim to localize immediately (that is, directly) in terms of hand-movement or visual imagery or head-movement, and not by any

character in the sound itself. G. has had more practice than the other observers. It is an odd confirmation of the automatism of her localizations that she places the sound more accurately when she is not paying very good attention.

It would be rash to infer that the imagery of localization affects its accuracy. Two of the more accurate observers localized in tactual terms and one visualized; of the less accurate observers, two visualized and one could give no clear account of herself.

The largest number of large errors were made by S.; the wildest and most anomalous errors were made by Cs. Cs. was the only one of the six who often confused the median and the sagittal arcs. The most perversely consistent errors were made by G., who habitually in her later work took B for F, BU 36 for U, and FD 36, a point practically in her lap, for some point in the arc BU. In the last (verbal) series she might have had 50 per cent. of right cases if she had not persisted in thinking of the horizontal plane as at the level of her neck, and R and L as straight out from the corners of her eyes, a natural error in an eye-minded subject. This stereotyping of certain tendencies may be another indication of automatic localization.

In the third and last place, the effect of suggestion must be considered. For a time, an attempt was made really to deceive the observers. They were flatly told that sounds would be given in a certain quadrant or hemisphere. A few of them detected the deception but many did not. To avoid this inequality of conditions for results which would have to be massed, the subjects in the later experiments were told to 'keep thinking' of a certain definite point.

The following table contains results from 64 subjects tested with the Titchener apparatus. Suggestion may or may not have been effective if a sound is both given and localized in the quadrant or hemisphere or at the point suggested. Such cases are marked with the sign ?. When the suggestion and the reality do not agree, suggestion is perhaps effective if the localization is in the direction of the suggestion and certainly ineffective if it is not. The first class of cases is marked + and the second — . In these experiments the points were evenly dis-

tributed between the right and left, front and back, and upper and lower hemispheres, and the same number of points were submitted to each of two opposite suggestions.

From these figures, it is clear that one effect of suggestion is to reduce the total number of right cases. To be sure, a rather larger percentage of cases are correct with right than with wrong suggestion. In general, however, the number of right cases with suggestion falls below the normal 11 per cent.

It is clear, also, that in the directions FB and BF, errors in accordance with suggestion are slightly more numerous than errors in defiance of suggestion. This statement holds true for errors upward and downward. It does not hold for errors to the right and left.

It looks, farther, as if suggestion diminished the normal tendency toward 'backward errors.' Moreover, in the earlier experiments with the Harvard apparatus, 'backward' suggestion seemed actually to produce a certain amount of accuracy. Here, when the arc FU was suggested (in 295 localizations) there were 4 per cent. of forward errors of over 90° in the median plane, whereas when BU was suggested (in 320 localizations) there were no backward errors of over 90°. On the other hand, in defiance of the suggestion BU, there were no errors of over 90°, whereas in defiance of the suggestion FU there were 10 per cent.

With the Titchener apparatus, the points most correctly localized by unpracticed observers, both with and without suggestion, were B, R and L. With the Münsterberg apparatus the maximum of accuracy shifts with suggestion from RB 60 and LB 60 to R, RF 60 and LF 60.

On the basis of figures which cannot be given, it is clear, finally, that suggestion most influences upward and downward errors. A given suggestion is not especially apt to produce corresponding errors, but any suggestion greatly increases the number of errors upward and downward. Errors forward and backward and to the right and left are actually fewer with suggestion than without it. The number of wrong cases with suggestion is swelled by the many upward and downward errors. Without suggestion, there are about as many of one as

TABLE IV.

SHOWING THE EFFECT OF SUGGESTION UPON UNPRACTICED OBSERVERS.

Suggestion.	Cases.	Cases with Correct Suggestion.	Right Cases, %.	Cases with Wrong Suggestion.	Right Cases, %.	Direction of Error· Cases in Percent.													
						FB.							BF.						
						Under 72°.				Over 72°.			Under 72°.				Over 72°.		
						?	+	—	Total.	+	—	Total.	?	+	—	Total.	+	—	Total.
Quadrants FU and BU.	590	177	6	413	8	5	15	11	*31*	8	6	*14*	7	11	4	*22*	3	2	*5*
Hemispheres U and D.	220	88	10	132	9				*21*			*10*				*26*			*12*
Points F and B.	300	30	30	270	7		12	12	*24*	6	6	*12*		14	11	*25*	4	2	*6*
Points R and L.	80			80	15				*20*			*23*				*14*			*1*
Points U and D.	120			120	8				*26*			*29*				*15*			*.8*
No suggestion.	1442	Right Cases, 11%.							*23*			*23*				*29*			*1*

of the other (of errors UD, 32 per cent. under 72° and 2 per cent. over, and of errors DU, 34 per cent. under 72° and 1 per cent. over, in the sets of results represented in Table IV.). With suggestion, upward errors, large and small, are in the lead.

On the whole, the negative effect of suggestion seems more marked than the positive. Suggestion seems to disintegrate the localizing function. It lessens the tendency to a given sort of error and yet reduces the number of right cases. The thought of any special direction, especially if there have been conflicts between suggestion and reality, seems to produce a kind of paralysis, like that of the famous centipede who, when the toad for fun said, 'Pray, which leg comes after which?' then 'lay distracted in a ditch considering how to run.'

Conclusion.

The following conclusions are suggested for future verification:

1. The perception of the direction of a telephone-click is not usually based on the consciousness of timbre, or of intensity or of pitch, or of any kind of place-mark or space-value in the sound itself.

2. Timbre and intensity criteria develop with experience in auditory localization, and seem in a measure to presuppose it.

3. Cutaneous impressions about the ears and head do sometimes serve as factors in the localization-consciousness.

4. Auditory localization is a rough counterpart of cutaneous localization. It proceeds originally by reflex head- and eye-movements which drop with practice.

The immediacy of auditory localization, if a real directness, cannot well be explained except by the lapsing of reflex movements. Moreover, in ordinary life, we are frequently finding ourselves and others turning the head from side to side to localize some faint sound. Apart from such obvious considerations, the following points are noted as collateral evidence for the fourth conclusion.

(1) Suggestion has no very marked effect on strong tendencies in the perception of direction.

(2) On the other hand, it hinders accurate localization, as thought about an automatic muscular coordination hinders the process.

(3) Unpracticed observers tend to localize sounds behind them. This tendency is perhaps explicable on the basis of serviceable reflex movements in response to a noise.

(4) Alleged immediacy in localization is coupled with relative accuracy in the cases carefully examined.

CORRELATIONS AMONG PERCEPTIVE AND ASSOCIATIVE PROCESSES.

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It has been the habit of psychologists to use the words 'memory,' 'attention,' 'delicacy of discrimination,' etc., as if they referred to general mental functions, and the words 'quickness' and 'accuracy' and 'ability,' as if they referred to general mental qualities. But any consideration of the nervous basis of mental life or of the patent facts of human nature suggests that *à priori* it is more rational to look on the mind as a multitude of particular capacities, particular associations and particular acts, all of which may be highly independent of each other. And the studies of Thorndike and Woodworth[1] on the effect of training, of Wissler[2] on the correlation of mental tests and of Norsworthy[3] on the correlation of school abilities, give actual evidence that such is the fact.

The extent to which any one mental trait presupposes any other is in fact in every case a matter to be actually measured. We cannot tell beforehand what the relationships are. The present study gives measurements of such relationships in the case of a number of functions, all of which depend upon quickness and accuracy in associating certain thoughts or acts with certain percepts either directly or indirectly through other ideas which the percepts call up. The associations were in every case such as involved (in the subjects tested) attentive selection of correct and purposive inhibition of incorrect ideas. They were controlled associations. The functions in question were much more alike than were those examined by Wissler. We

[1] PSYCHOLOGICAL REVIEW, Vol. VIII., Nos. 3, 4 and 6 (1901).
[2] PSYCHOLOGICAL REVIEW, Monograph Supplement No. 16 (1901).
[3] *Journal of Pedagogy*, Vol. XIV., No. 1 (1901).

have examined the relationships between functions in an extremely favorable case.

The results reinforce the evidence showing that functions apparently closely similar may really be to a large extent independent specializations. For instance the ability to call up quickly the opposites of 'good,' 'rich,' 'heavy,' etc., is by no means identical with the ability to call up quickly the letters coming before c, k, t, etc., or with the ability to call up quickly the answers to 7 + 4, 11 + 9, 20 + 6, etc. 'Quickness of association' as an ability determining the speed of all one's associations is a myth. Quickness in noticing words containing the two letters r and e does not to any appreciable extent involve quickness in noticing words grossly misspelled, nor does accuracy in the one involve anything like equal accuracy in the other. The 'attention' and 'discrimination' required in the two cases must therefore be different things.

Our results also suggest the possibility of clearly defining the classes of functions which we may expect to find closely related. For in the tests involving quickness and accuracy in purposive associations there is much closer dependence than in the tests involving quickness and accuracy of perception, though to the speculative psychologist the latter would seem to be cases of the same function.

It may be added that both of these conclusions are supported by the results of many other correlations not yet worked out in detail and so not presented here.

The importance to general psychological theory of measures of the relationship of different mental functions is obvious. Where introspective analysis fails to discover the exact structure of a mental process, the study of its relationships with other better known functions may succeed. Moreover the whole question of the influences of heredity, maturity and training will be illuminated by a knowledge of the necessary bonds and interactions of the different mental processes.

The tests were all given by one person; they were given to 160 boys and girls in the eighth grade (the highest) and to 80 in the fifth grade. Care was taken that in all the tests all the pupils

had the same conditions of explanation, incentive, time, etc. The tests were the following:

1. To mark every mispelled word in this passage (time, 180 seconds):

I percieved, about four years ago, a large spiider in one korner of my room, makeing its web; and through the maid frequentely leveled her fatale brom against the lobors of the little anemal, I had the good fortoone then to prevente its distrucsion, and, I may say, it mor than paid me by the intertainement it aforded.

In thre days the weeb was, with encredable diligence, compleeted; nor could I avod thinkeing that the insect seemed to exult in its new abode. It ofen treversd it round, and exsamined the strenth of every part of it, retierd into its whole, and came out very ferquently. The first inemy, however, it had to inconter was another and much larger spidur, which, having no web of its owne, and haveing probibly hexausted all its stock in former labors of this kind, came to invaide the prouperty of its nieghbore.

Soon a terreble encounter ensooed, in which the invader seemed to have the victorie, and the laborius spider was obleeged to take refug in its hole. Upon this I perceived the victer useing every art to draw the enemey from his strongholde. He seemed to go of, but quicklie returned, and, when he found all arts vane, began to dimoilish the new web withoute mercy. This broght on another battle, and contary to my expextations, the laborious spider became conckeror, and fairly killed his antagonist.

Nou in pieceable possession of what was justely its own, it awated three days with the uttmoste impatients, repairing the breeches of its web, and taking no sustenance that I could perceive. Ate last, houever, a large blue fly fell into the snaire, and strugled hard to get lose. The spider gave it leeve to intangle itself as much as possible, but it seemed to be to strong for the cobwebe.

I must own I was grately serprised when I saw the spider imediately sally out, and in lese than a minite wheave a new nett around its capthive, by wich the moshun of its wings was stoped, and, when it was faircly hampered in this maner, it was siezed and druged into the houle.

In this manner it lifed, in a precarious staite, and Natcher seemed to have fited it for such a life, for upon a singl fly it subsested for a weak. I put a waspe into the neat, but the spider sit it free.

2. To mark every word containing r and e in this passage (time, 60 seconds):

Then the carelessly dressed gentleman stepped lightly into Warren's carriage and held out a small card. John vanished behind the bushes and the carriage moved along down the driveway. The audience of passers-by which had been gathering about them melted away in an instant leaving only a poor old lady on the curb. Albert was sadly striding across the field stopping now and then to look back at the village or staring at the bit of paper which he still held tightly grasped in his left hand. He wished to keep his appointment; nevertheless he felt that he ought to go back to Cromley to ascertain in what direction the stranger might be going. The highway lay on his left seeming

for all the world like a dusty ribbon. Before him gleamed the spire of Relton church. His thoughts flew to his former roving in the days when Clayton and he were working for Fleckmer. "What an insatiable greed for power that man had," he thought, "I on the contrary have really no ambition." He laughed bitterly. "Hopelessly inactive I truly am." A partridge flew up ahead of him. He tried to hit it, but merely knocked off some leaves.

3, 4 and 5. To fill out the blanks A, B and C, thirty seconds being given for A and also for B and fifteen seconds for C.

A.	B.	C.
Write as fast as you can beside each of the words in the column a word that means the opposite thing from it.	Write as fast as you can beside each of the words in the column a word that means the opposite thing from it.	Write as quickly as you can beside each letter in the column, the letter that comes *before* it in the alphabet.
stupid	good	f
hard-working	outside	k
strong	friend	s
sane	quick	p
obnoxious	tall	w
foolish	big	l
handsome	loud	e
adroit	white	r
superior	light	d
loquacious	happy	o
rapid	false	v
generous	like	j
straight	rich	n
separate	sick	t
up	glad	h
always	thin	
joy	empty	
high	war	
obscure	many	
proud	above	

6. To do the following examples (120 seconds):

ADDITION EXAMPLES.

17	26	27	72	23
42	51	24	14	47
38	47	83	39	86
91	82	19	81	54
54	63	45	26	36

17	42	38	91	36
26	51	47	82	26
27	24	83	19	45
72	14	39	62	63
23	47	86	54	54

41	53	67	78	86
52	67	86	37	32
86	34	23	96	44
23	78	45	72	36
35	19	67	23	68

45	52	19	45	23
13	86	78	67	72
68	23	67	78	36
77	35	23	37	68
86	67	86	96	39

In the misspelled word test each individual was given five scores: (1) The number of lines covered; (2) the number of words marked that should have been marked; (3) the number of words marked per line; (4) the number of words marked that should not have been marked; (5) a mark for his general efficiency in the test, obtained by finding in 2 and 3 his distance above or below the average for his grade in terms of the average deviation of the grade from that average, adding the two distances together, and adding or subtracting a small amount to the figure thus obtained in the case of those who were much above or below the average in the number of words incorrectly marked. Similar scores were given in the case of the R-E test.

In the hard opposites (A), easy opposites (B), and alphabet tests (C), each individual was given five scores, namely, (1) the point reached in the test, (2) the number done, (3) the number of errors, (4) the percentage of errors to the number done, and (5) a general score for efficiency calculated differently in the three cases. The method of calculation with the hard opposites was to subtract twice the number of errors from the number done, multiply the product by two and subtract the number of omissions; with the easy opposites do the same, save that three times the number of omissions were subtracted; with the alphabet test twice the number of errors were subtracted from the number done, and a correction made in the few cases where there were omissions according to the nature of each case. These scores of efficiency are to some extent arbitrary and are

possibly unfair. They are not of much importance in the discussion.

Each individual was also given a score for the total number done in all three of these tests, a score for the total number of errors, a score for the ratio of total errors to total number done, a score for the total number correct, and a score for total efficiency obtained by adding the efficiency scores in hard opposites and alphabet to one-half the score in easy opposites.

The scores given in the addition test were (1) number done, (2) number of errors, (3) number of errors per example, and (4) a score for general efficiency obtained by subtracting one-half of (2) from (1).

We have thus for each individual a record like this:

Grace Alexander.

Misspelled Words.

Number of lines	31
Number of correct words	75
Words per line	2.4
Incorrect words	2
Efficiency	+1.3

RE.

Number of lines	9
Number of correct words	11
Words per line	1.2
Incorrect words	0
Efficiency	—.68

	Hard opp.	Easy opp.	Alphabet.
Number reached	9	6	3
Number done	7	6	3
Number of errors	1	1	0
Percentage of errors	14	17	0
Efficiency	8	8	3

Associative Tests Combined.

Total number done	16
Total number of errors	2
Total percentage of errors	12.5
Total number correct	14
Total efficiency	15

ADDITION.

Number done	9
Number of errors	2
Percentage of errors	.22
Efficiency	8

The relationship in the case of the individuals tested between the two abilities measured by any two of these tests will be represented by any figure or diagram or statement that shows how far variations in one of the abilities go with similar or opposite variations in the other. We shall use a single figure for that purpose. Its meaning will be clear from the following derivation in one case:

The relationship between the amount done in addition and the total amount done in the three other association tests.

15 students doing the least in addition, namely from 3 to 6, got the following scores in the three other tests: 2 got 10, 3 got 12, 1 got 13, 3 got 14, 1 got 16, 3 got 17, 1 got 19, 1 got 20. They averaged 14.5. If these fifteen who did the least in addition had been the fifteen doing the least in the association tests they would have obtained the following scores: one 6, one 7, two 9, six 10, four 11 and one 12, and would have averaged 9.9. The general average in the total for the three association tests was 17.6. The lowest 15 in addition were thus 3.1 below the average in the total for the three association tests. If correlation had been perfect, *i. e.*, if an individual's rank in one exactly corresponded with his rank in the other, they would have been 7.7 below. The degree of correlation for them was thus 40 per cent.

36 students doing 7 in addition got in the three other tests, etc.
28 " " 8 " " etc.
35 " " 9 " " "
24 " " 10 " " "
18 " " from 11 to 16 in addition, etc.

Data for all these groups similar to those given above for the lowest 15 are presented in Table A.

TABLE A.

RELATION BETWEEN AMOUNT DONE IN ADDITION AND AMOUNT DONE IN THE THREE COMBINED ASSOCIATION TESTS.

Each figure in the table records that that many individuals did in addition the amount *under* which the figure stands and in the three combined association tests the amount *opposite* which the figure stands.

	3	4	5	6	7	8	9	10	11	12	13	14	15	16
6					1									
7							1							
8														
9					2									
10				2	3		1							
11					1	1	1	1						
12		1		2	1		1				1			
13	1				3	1	2							
14			1	2	3	3	1	2						
15					3	1	3	1	1					
16				1	7	2	6	2	1					
17			2	1	5	5		2		1				
18					2	2	1	3	1					
19				1		3	4	1			1			
20			1		2	4	4	1	2					
21					2	3	2	5	1					
22					1		4	2		1				1
23							1			1				
24							1	1		2				
25						1				2	1			
26						1	1	1	1					
27							1	1						
28								1						
29														
30														
31						1								

	3–6	7	8–9	10	11–16	
Number of cases.	15	36	63	24	18	Total 156.
Average amount in association tests.	14.5	15	18.1[1]	19.5	20.8	General average 17.6
Average if perfect correlation.	9.9	14	17.8	21.3	25.5	
Actual deviations from general average.	—3.1	—2.6	+.5[1]	+1.9	+3.2	
Deviations of perfect correlation.	—7.7	—3.6	+.2	+3.7	+7.9	
Percentages of correlation.	40	72	—[1]	51	41	Average correlation 51%.

[1] These cases so near the average should be omitted in computing the general relationship, since they give a different result if grouped differently.

The average of the percentages of correlation for the different groups, gives a sufficiently though not the most accurate single figure to express the closeness of relationship between the two functions. Perfect correlation would be represented, of course, by 100%, utter independence by 0%, and antagonism in the two functions by a figure with a negative sign.

The following relationship was worked out by the method just described. When there are two figures for the relationship the upper one refers to the relationship found in the 8th grade pupils, the lower one to that found in the 5th grade pupils. Where only one figure is given, it refers to the 8th grade pupils alone.

	Per Cent. of Correlation.
Misspelled words marked (2) and R. E. words marked (2). Quickness (and to some extent accuracy) of Perception	0 0
Misspelled words per line (3) and R. E. words per line (3). Accuracy of Perception	16 25
Misspelled words (efficiency (5)) and R. E. words (efficiency (5))	None or slight.
Hard opposites (number done (2)) and easy opposites (number done (2)). Quickness of association	50 50
Hard opposites (number done) and alphabet (number done). Quickness of association	22 19
Alphabet (number done) and easy opposites (number done). Quickness of association	40 50
116 cases from another school	41
Hard opposites (percentage of errors) and easy opposites (percentage of errors). Accuracy of association	20
Hard opposites (percentage of errors) and alphabet (percentage of errors). Accuracy of association	10
Alphabet (percentage of errors) and easy opposites (percentage of errors). Accuracy of association	40
Hard opposites (efficiency) and easy opposites (efficiency)	40
Hard opposites (efficiency) and alphabet (efficiency)	34
Alphabet (efficiency) and easy opposites (efficiency)	60
Association tests (total number done) and addition (number done)	{ 50 { 20
Association tests (total percentage of errors) with addition (percentage of errors)	None or slight.
Association tests (efficiency) and addition (efficiency)	48
Association tests (total number done) and R. E. (number of words marked)	None or slight.
Association tests (total number done) and R. E. (lines marked)	8 or less.
Association tests (total number done) and misspelled words marked	20
Association tests (efficiency) and R. E. (efficiency)	None or slight.
Association tests (efficiency) and misspelled words (efficiency)	50
Addition (number done) and R. E. (number of words marked)	None or slight.
Addition (number done) and misspelled words marked	20
Addition (efficiency) and R. E. (efficiency)	None or slight.
Addition (efficiency) and misspelled words (efficiency)	50

DISCUSSION AND REPORTS.

IMITATION.

May I offer one or two remarks on Professor Baldwin's references to my views in your issue of January, 1902, which, owing to my absence from England, did not reach me until March? I am persuaded that when we differ on psychology Professor Baldwin is almost certain to be right. And yet I cannot feel that my difficulties are met by what he has said; and I should like to try and make my point clear.

First, I will restate my difficulty in general terms. The question is, as I understand it, which of two suggested principles helps us most in explaining that transference and operation of ideas by which men are social. Professor Baldwin says 'imitation'; I say 'logic.' Neither of us, I believe, holds reflective consciousness to be necessary to the operation of the principle. For him, this operation *is* imitative process, and results in producing, transferring or realizing resemblances of copies—on the whole, in repetition. For me, it *is* logical process, and results in producing, transferring or realizing systems of connected differences—on the whole, in organization. His principle works by similarity; mine by identity in difference. My difficulty is that I cannot see how development into a group of interrelated elements—something organic, a design, or pattern, or situation—can be got by imitative process proper. I admit and maintain that every mental process tends to such development; but this seems to me to show, not that pure imitation can organize, but that mental process cannot be purely imitative.

Is it a verbal question? Has imitation been construed to cover logical process? If so, that explains our difference. Or (a somewhat different case), is 'imitation,' taken in a certain technical sense and with certain additions, proposed as the scientific or psychological *explanation* of logical development? I suspect something like this to be meant. But if so, I think this is more than a verbal question, so long as imitation retains anything of its primary meaning, viz., the production of resemblances. My view is that imitation is a case of logical process, in which differentiation of the copy, always present, is disregarded *ad hoc*. To build up logical process out of imitation seems

to me, therefore, to be a topsy-turvy arrangement. It seems to me to found the theory of a great subject on an extreme subcase, and on only one half of that.

1. To begin with, Professor Baldwin seems to urge that imitation is scientifically useful and identity in difference is not—to make our difference, in short, one of science versus philosophy. Now, I believe I know what is meant by a working hypothesis; but I can see no advantage in its being further wrong than it need be. It is obvious (pp. 57–58) that 'identity in difference' solves no question by being merely mentioned. But the same, surely, must be true of 'imitation.' If, however, either is to be applied in analysis, then I think that the 'sociologist, statistician, etc., etc.,' would gain, by using the former, a higher degree of delicacy in their solutions, and perhaps the avoidance of serious blunders—such as I have pointed out in LeBon's confusion of a crowd with a state, and Durkheim's separation of penal and civil law. Space forbids me to analyze the instances which Professor Baldwin adduces—the cotton-gin and the lynching party;—but if it is implied that a lynching party is pure 'follow-my-leader,' or the cotton-gin pure invention, I suspect that the analysis would bear revising in the light of a less crude principle. Even in biology it is conceivable that another point of view might be helpful in discussing the degree of generality of inherited characters or tendencies. Might not, for instance, that which appeared as drink in the father appear as poetry in the son, simply by reaction to different surroundings in life, and without prenatal 'variations'? If so, would not an error of method be committed by looking for a special cause of variation in such an instance? Such considerations are not foreign, surely, to post-Darwinian controversy,[1] and it was Darwin's own care and skill in defining the general nature of certain inherited tendencies which first impressed me with the need for caution at this point ('Origin of Species,' 6th ed., p. 209).

I have purposely, for shortness' sake, given myself away *prima facie*, by saying that my principle is 'logic.' It may be retorted at once, 'Logic cannot be a principle in psychology.' But this would suit me; for I only ask to know where we are; that is, what consequences will be drawn from this assertion. Logic is the shortest expression I can think of for the operation of ideas in organizing wholes of thought. If, in psychology, you cannot call it logic, still you must deal somehow with this operation of ideas, and I want to know how it is dealt with in the theory before us. Is it explained by

[1] *E. g.*, Weismann, Biological Memoirs, Clarendon Press, 1889, p. 393.

imitation alone, or by imitation plus something else—selection, perhaps—or is it, as I suspect, to a great extent omitted?

2. The imitation theory derives an undue advantage from the application of such terms as 'general' and 'collective' to the 'general will.' This favors a loose notion that the general self consists of the common elements, roughly called resemblances, among a number of wills. But in principle the will is always a system, and the contribution of each unit is individual, related organically, and not as resemblance, not as a common element, to that of the other individuals. This necessity, as I understand, Professor Baldwin accepts and maintains in his doctrine of the 'thought situation' and publicity. But with the recognition of this necessity, imitation ceases to be a plausible explanation, except in a subordinate sense which I will state directly. *Prima facie* imitation can never give the thought of a situation. A thought situation means an idea of relation between things and persons variously connected, but so differing as to form a coöperative whole. I do not see how imitation as such can ever produce an idea of the definite difference and correlation, within a situation or pattern, of the imitator and the original. For this it is necessary that a principle or content, which may be suggested no doubt by any object of imitation, should take on a different but corresponding shape in the coöperating units; and that the different shapes should be held together, and should operate in conjunction, as elements of the whole content which now includes these differences. The value of pure imitation must depend, so far as I understand, on the complexity of the individual's nature. If the situation involves a child and a teacher, then, say, for a third person dealing with it, it will be a help to have reinforced, as if by resonators, the child nature and the teacher nature within himself, through an experience which may fairly be called imitation. But the grasp of the relation between the persons involved belongs to the mental process as a whole, and not to its imitative side.

I do not see the initial difficulty of obtaining such a grasp, which is the condition of a general will or self. It is implied in mental process from the beginning, through the principle that contents operate as universals, assuming differences in accordance with the details through which they are reproduced. As regards the participation of different intelligent individuals in one such grasp, the true principle seems to me to be that an idea normally operates throughout several intelligences just as within one. It is obstruction and lack of communication that is, in principle, abnormal. Communication depends ultimately on logic; language and concerted action are incidental

cases of it. I mean that in all cases the ultimate nerve of intercourse is that, according to a nature common to all concerned, one thing follows from another—it may be act, speech, thought or feeling. The thing so following, whether in myself or in another, I equally recognize as the continuation and completion of my thought. Of course I constantly fail to understand, but in as far as I do understand, whether through common action, or language, or perception, or feeling, that is what my understanding means. I want, perhaps, food, rest, tendance in sickness, a railway ticket, a laugh, sympathy, a fight. I get what I want, it may be by words, signs, looks, or by simply presenting myself. This is all incidental. The nerve of the whole process is that given the data, including their own resources, other minds bring out, in a form prescribed by their powers, the conclusion at which mine is aiming. The point is, that it is a *conclusion*—that is, something new, and different from the premises, and consisting for the other minds not in a copy of my state or action, but in another state or action definitely relevant to it in consequence of a common principle which connects the two. Here we have the essentials of the thought situation; and its development into explicit unity is only a matter of the degree of reflective consciousness.

3. Has Professor Baldwin at last (p. 67) done justice to logic by his doctrine of selective thinking? It appears to be intended to supplement the doctrine of imitation by showing how, in applying a copy to new conditions, variations suitable to those conditions arise. It should be then, in fact, a theory of the logical process as above stated. And such a theory, in psychological terms, would be welcome.

The question of principle seems to be whether selective thinking means selection of workable combinations out of indeterminate variations, by trial and error, or construction of a determinate adapted variation by logical process. The prerogative of reason surely is to ascertain, without trial and error, that is, without actual working upon things, what adaptation is workable in a given situation. I will take calculation as a very simple instance. When I have calculated how many trees are needed to plant an acre of ground at a given distance apart, the adaptation is fully determinate. There is no room for variation or for selection. "Selection of actions which 'work' in a given situation" would most naturally mean the sort of thing which we are told a dog does in trying to open a gate, viz., selection by trial and error. If it means this, it omits the essence of calculation and inference, in short, of reasoning. If it is to cover the facts of reasoning, then the term 'selection' must be slurred, and the meaning of

the term 'work' or 'workable' extended to cover all the ways in which inference can be confirmed. But if so, we are making logic its own explanation. 'Actions which work,' 'workable combinations,' will include the results of theories which 'work,' that is, which agree with experience in any form, practical or cognitive.

Thus, when we hear that 'thoughts are the counterparts of former adapted actions' we may interpret this in two ways. It may mean that a thought is the memory or at least the effect of a lucky hit, such as a dog makes in lifting the latch of a gate; in this case there was no principle of adaptation in the action, and the thought, as dependent on such a process, has none either. Each step depends on trial and error, and advance depends on a succession of lucky hits. Or it may mean that a thought is a phase of adapted principle, carrying on in a new situation the idea which gave a successful conclusion before. But in this case thought depends on previous thought of the same kind; the variation becomes predetermined; selection vanishes, and with it the prerogative of actual working on things as a test. For all thought whatever is a principle at work, in the sense in which calculated action is so; and whether the conditions to be met are materially present, or only present to knowledge, can involve no difference of any kind to the nerve of the process. It is only in trial and error that practice on material things has a prerogative over inference. When we come to calculation and reasoning the special import of material action disappears, and it becomes one among many forms of cognition, all of which are tested by 'working' in the wider sense. Take the instance of building a house (I quote the passage):[1] "I should say that the plan of the whole is made up of parts each of which is taken imitatively from other houses or plans of houses, or selected out by the owner himself from alternative variations of thought, by the process of getting new workable combinations, which is indicated above."

What is meant here by 'getting new workable combinations'? It may mean (i) that you try putting together parts borrowed from other examples—to be consistent, you should try them in actual bricks and mortar—and when you find a combination that does not tumble down, you accept it, like a child building with wooden bricks, or perhaps a workman by rule of thumb. This would be true selection, of variations taken imitatively, according to what is found workable in material action.

But the moment you start even to draw a plan, or to calculate dimensions, not to speak of reasoning to a general effect, something

[1] PSYCH. REVIEW, IX., p. 67.

quite different begins to take place.[1] (ii) A whole comes into being, whose nature enters into and affects the nature of every part, and by the necessity of the case the logical unity, which in this case is called design, asserts itself throughout. It is true that there must be suggestions; a modern house could not be built if no house had ever been built before. It is, I think, not true that the suggestions can be taken imitatively, though design of a low type may attempt something of the kind. Every one must be recast as it is taken up in the whole. Now the point is that in this case the expression 'getting new workable combinations' has lost its distinctive meaning as an explanation of thinking by something else, viz., choice in action. 'Workable' no longer means what you find to work by actual trial, but merely what is workable in a logical sense, *i. e.*, what is self-consistent or even what is satisfactory in the world of architectural experience. Selective thinking, in short, is an improper phrase, because thinking as such is selective and more than selective, being selective, so to speak, by construction of determinate adapted variations. Professor Baldwin has a leaning towards determinate variations.[2] I believe that this is the crux of the whole matter, and that if the origin of determinate variations is fairly considered the idea of selective thinking must go. I do not believe that an explanation of logical process can be built up on imitation plus selection, and the facts seem to me to be in the main omitted by the imitation theory.

4. I will not call Professor Baldwin an associationist; for no one should be called by a party name which he disclaims. But I will explain what I meant by saying that I was not sure whether to call him so. I simply meant that he seems to me to deny or to neglect logical process. Thought, as I understand him, is essentially reproduction of resemblances plus acceptance of variations. In its own nature, thought has no synthetic unity, but only repetition. All unity is derivative from the test of action; it is motor unity, synergy, unity of action. Thought repeats anything that is offered it; it is action that solidifies in a system that which is coherent on actual trial, and shaves away the superfluous or discordant. Now I see the vraisemblance of this. For good logical reasons material action is an incessant induction. But the crux of our question is, *why* is material action an excellent test of thoughts? My answer is, because in assignable respects it is a very strong case of thinking with verification. But if

[1] Cf. Stout, 'Anal. Psychology,' II., p. 60. I do not know whether, as Professor Baldwin seems to hint (p. 68), Mr. Stout has abandoned the essence of the view here expressed.

[2] 'Social and Ethical Interpretations,' p. 96.

you make thought borrow its unity and consistency, having none in itself, from what is found to succeed in action, then I no longer see why we should respect these qualities or treat them as guides to truth or reality. Action no longer reveals a principle that pervades experience. On the contrary, experience is erected on *de facto* successes in action, with the origin of which no nexus or principle in experience has anything to do.

This view, then, seemed to me associationist because it made thought capable only of joining or reproducing the given, and not of carrying out a principle through determinate variations, adjusted to presented details, into an organized system. If, to do this, thought rests on action which is not thought, then action in doing it rests on nothing.

I believe I see the strength of Professor Baldwin's view. It is that in thinking everything comes from somewhere. We make nothing 'out of whole cloth.' Therefore, it is natural to speak of imitation and selection. But if we can make nothing new, on the other hand we can take nothing as it stands. We take always for a reason and in carrying out a connection; and this connection modifies what is taken. As taken, therefore, the elements of our thoughts, though not new, are original. They rest on a reason, not on mere trial.

These are the difficulties which I wished to have cleared up. I am quite ready to believe that I have missed Professor Baldwin's intention. But I am very strongly convinced that his view needs at least restating.

BERNARD BOSANQUET.

MR. SUMNER'S REVIEW OF THE PIPER REPORT.

It is probable that most of those who read Mr. Sumner's remarks on my Report of the Piper case will not read the Report itself. I therefore ought to correct some of his misapprehensions and misrepresentations. I shall not enter into any defence of the hypothesis entertained in that Report. This is not called for in the PSYCHOLOGICAL REVIEW, which is devoted to more important matters.

But first let me express my perfect agreement with the Editor, Professor Cattell, in the view that 'the phenomena are not suited to scientific investigation until the possibility of fraud has been excluded.' This statement is so true that I think it applies to all experimental psychology, and perhaps the results in some cases would be much more respected if we had been shown how frauds had been excluded from the work. But I am glad to see the maxim recognized even at

this late date, though I would not be surprised to discover that even psychical researchers had felt some fear that it would some day be suggested as important. They will certainly congratulate all scientific men on the discovery of it and for the solicitude that would keep us from going astray. But I must emphatically differ with the Editor on the matter of placing psychical research publications in the hands of sympathizers with it, as this might compromise the reputation of the REVIEW and encourage the favorable notice of ideas that might better be stamped out. I rather suspect that it would conduce more decidedly to scientific truth to put such matters into the hands of men who are able to teach us something on such problems. I do not read one per cent. of the reviews of books for the very reason that the maxim here announced by the Editor has so generally been followed. Moreover, if psychical research cannot stand the animadversions of its intelligent opponents it is not worth much.

Let me now enumerate the misapprehensions and misrepresentations of my critic, Mr. Sumner.

1. His very first allusion to my remarks on *suggestion* as an explanation at least apparently implies that I admitted it as a *fact*, when I was very careful to say that it was 'a conceivable explanation in a few isolated instances.' This he quoted, but does not represent rightly.

2. Mr. Sumner apparently does not note, he certainly does not call any attention to my remark that suggestion is worthless unless it has some applicability to the whole of the record, and particularly to *incidents*, in not one case of which has he mentioned an instance of suggestion. Whatever theory is adopted, even that of fraud, must have some unity and simplicity and not be a system of 'cycles and epicycles' of fishing, guessing, suggestion, telepathy, clairvoyance, wireless telegraphy analogies, secondary personality in a rather supernormal form, and what not. After having admitted the supernormal in the case Mr. Sumner ought to have seen that suggestion has no other place in the matter than a possibility, in so far as evidence is concerned.

3. My question about my mother does *not* 'obviously imply' her decease, as Mr. Sumner says it does, unless you assume that such a question can have but one motive. I tried to trick the 'communicator' with the same question about my brother George, who is still living, and I finally and spontaneously got the right answer with an interesting statement about my question. Mr. Sumner's objection here ought to have been that it was a case of *guessing*, not one of

suggestion. Besides he might have noted what I said (pp. 146–7) about the incidents relative to my mother.

4. In regard to the name McClellan, which has a more plausible claim to explanation by suggestion, I may say that I once wrote out a note indicating precisely the circumstance that Mr. Sumner mentions, but when I observed that the mistake in the name on one day was committed again twice the next day (pp. 427 and 429) after I had pronounced it the previous day and before it was spontaneously corrected in a dramatic manner, I saw that it was not properly a case of suggestion at all, and so decided to omit the note. Besides, why did not Mr. Sumner try to indicate how 'suggestion' worked in regard to the name Cooper, also uttered directly in connection with the name McClellan?

5. A much more serious error of Mr. Sumner is the fact that he reverses the chronological order of the facts in the record apparently in order to make out a plausible case of suggestion. He refers to this mention of the name McClellan before it was correctly given after he had indicated that it had been given with approximate correctness. This gives the appearance of the 'suggestion' which the record does not support.

6. In regard to the name and relationship of my father and Mr. Sumner's remarks thereon, several things should be said. (*a*) Mr. Sumner does not quote *all* the 'recorded facts.' (*b*) He omits to mention what was said by Mrs. Piper as she came out of the trace. (*c*) He omits the most important part of what I said in my note about the mispronunciation of the name after I had uttered it. (*d*) What kind of 'suggestion' is it to have spoken the name correctly *twice before I uttered it at all?* (*e*) What kind of 'suggestion' is it to have pronounced it wrongly after I had pronounced it correctly?

7. In the case of the name of my sister Hettie, Mr. Sumner does not tell the reader what I myself said of it (p. 68), but speaks of it in the list as if I had not considered it amenable to the suspicion of suggestion. I actually stated that I deliberately helped in the getting of that name. I did this for reasons apparent in the experiments on the identification of personality where I refused to permit the sending of the communicator's name. Besides, the case is more probably amenable to the theory of guessing than that of suggestion.

8. The only instance besides the last in any way liable to the charge that it is explicable by suggestion is not mentioned by Mr. Sumner at all, though I intimated the fact in my note. It is the name Clarke for my uncle. Why not tell the reader the facts of the two cases?

9. Mr. Sumner speaks of the list, using my phrase, 'isolated instances,' in quotation marks, as if there were many of them in the record. All but one in his list (and this may not be an exception) are not instances of suggestion, but if objection be taken to them they should be called guessing, a much simpler theory. Then near the close of the review he says: "The information given is certainly (sic) derived in great part from suggestions received from the sitter," etc., *ipse dixit*. But not one iota of evidence for so positive and general a statement. He does not show how a *single synthetic incident* is due to the suggestion he intimates.

10. Mr. Sumner's solicitous fear that I have 'thrown scientific caution to the winds' he illustrates by quoting the record regarding incidents connected with my father's death, and then insinuates that they have no pertinence for a death from 'something like cancer of the larynx.' Possibly he did not read, but he certainly does not tell the reader a word of my very full notes (pp. 328–9, 356–7) showing the great pertinence of this very passage in all its confusion. Besides, if he had referred to what I had said about such confusion (pp. 214–238, 249, 280–285; especially 284–5, and 643–649) he would have understood, supposing that he knows half as much about secondary personality as he would have us believe, why I considered such incidents as 'remarkable.' But it seems to have been important to omit all reference to facts in order to give force to innuendos. I fully understand and respect perplexities regarding passages of this kind, but when criticism is indulged in I am entitled to a full and correct statement of my position regarding them and not a garbled account.

11. In the allusion to my reconstruction of a 'message' regarding a trip with my father Mr. Sumner does not tell the reader what I had said about this very reconstruction *after* I had made it, and that I had condemned it in as strong terms as he does, though I intimated that it was a possible view (p. 409). I spoke in strong terms of such a procedure. Mr. Sumner refers only to certain statements of an earlier date about the 'message' (p. 371). Neither does he tell the reader what I had said about the retention of earlier notes bearing upon just such incidents (pp. 19, 301–2).

12. Mr. Sumner commits the same sin in reference to the name *Bartlett*. He does not tell the reader that I spoke of the possible interpretation in much stronger terms than he had done (p. 404). Besides, when he says that "no such person (as Bartlett) was known by the 'spirit' in life," Mr. Sumner supplies the statement from his own imagination. There is no such statement of fact in the record.

My statement of the facts was a very different one. What would Mr. Sumner say if I remarked that *Bonner* was the name of the principal of the high school to which reference was made in the 'communications,' and who was probably the man with whom my father had talked about my brother?

13. I cannot understand why Mr. Sumner should adopt my sentiments about the last two matters without acknowledging them and then consider that he had presented objections to the case. This is unusual criticism. When his opinion is the same as mine I cannot see, outside of Hegelianism, how it can be opposed to mine.

14. I would like to have it explained why it is 'credulity' to believe in spirits, which are nothing more than the extension in time of a known cause, a human consciousness, and why it is not 'credulity,' on the same evidence, to believe in the supernormal, telepathy, etc., which is wholly an *unknown* principle of explanation. Why is it so scientific to believe in any inexplicable miracles whatever instead of perfectly explicable spirits which act according to known mental laws?

15. In saying that I have probably 'underestimated the histrionic possibilities of secondary personality,' Mr. Sumner neglects to tell the reader what I said about this very thing in referring to the trance personalities (pp. 153–4, 264, 265–6, 292). I distinctly asserted that we had to assume the very utmost in this regard and that the trance personalities must be assumed to be the secondary personality of Mrs. Piper from the standpoint of personal identity which was the criterion in my Report.

16. Mr. Sumner misapprehends my observation that the 'difficulties and objections' to the spiritistic hypothesis are to be found outside my record when he quotes from it the instances which he treats as objections. He ought to have seen from my discussion (pp. 214–238) that such instances as he refers to are treated as *favorable* to the hypothesis and not as opposed to it. They are phenomena that require to be made *intelligible*, but they are not 'difficulties and objections.' It is our knowledge of secondary personality and possibly of the supernormal independently of the Piper case that affords the clue to objections.

17. Mr. Sumner thinks that I have 'ignored the formidable array of difficulties presented by Mrs. Sidgwick.' I reply that I did not ignore them. I considered them in my general treatment of the subject. I had read Mrs. Sidgwick's paper, and have also seen the original records in nearly all the cases mentioned by my critic. But scientific procedure did not allow me to consider questions involved in records

not yet published, and when these are read it will be found that they do not present such 'formidable difficulties' as are here imagined. It is strange that Mr. Sumner should praise me for referring the reader to my detailed record where he could find how mistaken I was in my discussion, and then quote Mrs. Sidgwick's opinions and discussion without waiting to see the original and detailed records. He might have applied his own advice in the last sentence of his review, namely, 'await more data.'

18. In regard to Dr. Hodgson's sittings and the promise of my 'father' to give tests Mr. Sumner says: 'In spite of these promises, the subsequent statements are as confused and as far from the truth as before.' Not all of them. Mr. Sumner neglects to tell the reader that 70 per cent. of them were entirely correct, and 16 per cent. partly correct. His remark applies only to 14 per cent. (p. 119). It might have been well to inform the reader also that I had discussed the questions involved in this 14 per cent. (pp. 214–238).

19. Referring to expressions that were not characteristic of 'communicators,' and using the fact as an objection to the spiritistic hypothesis, Mr. Sumner neither tells the reader that I had specially remarked their non-characteristic nature nor hints that I had specially referred to them as objections to the telepathy which he is willing to accept on the characteristic facts. He simply refers to them in a way which implies that he had picked them out in his own reading, and tries to distort them into an argument against my theory without telling the reader that I had used them in favor of it because against telepathy.

20. In regard to the 'appearance' of various historical notabilities, such as Mrs. Siddons, Bach, Homer, Ulysses, etc., the statements about Phinuit, and Adam Bede, and about the 'spirit' who was alive and well, most of which were taken from Mrs. Sidgwick's paper, I would only repeat Mr. Sumner's maxim, 'Await more data.' As said above I have seen the original records and they do not present any 'insuperable objections' to one who understands secondary personality, as Mr. Sumner wishes us to believe he understands it, and the possibility that this is the mental condition of the 'communicator.' I had so emphasized this point (pp. 249, 284–5, and 643–649), and its bearing upon such cases ought to have been seen and mentioned as sufficient indication of what the theory was and upon what it depended for its full character.

21. The contradictions charged to Phinuit and implied of other personalities do not affect the question of their *existence*, but only their

reliability for telling the truth as to their identity. If contradiction disputed the existence of any being I fear that we should have no reason to believe in the present existence of the human race.

22. Mr. Sumner says: "The 'father' in these communications frequently mentions an 'Annie,' seeming to refer to this stepmother whose name was Maggie." This is not correct. It was not 'Annie' that was frequently mentioned with the apparent reference mentioned. I was careful to say that it was first 'Mannie' and then became 'Nannie' (pp. 47, 342, and 365–6). Either the typesetter or Mr. Sumner made, in this name 'Annie,' and Mr. Sumner did not correct it, but repeated it when the proofs were read, the very same mistake that was made in the 'communications' of my record and which I commented on in Note 95 (p. 536) and the obverse of which was committed at another time and commented on in an interpretation (pp. 231–235, and 536). If living normal persons make this kind of mistake regarding the identity of any one, is it surprising that discarnate spirits might confuse their own identity and that of others when it is supposed that something like secondary personality is their mental condition while communicating? Whether this hypothesis is true or not, it is a part of the theory defended on the evidence of the record (pp. 643–649), and covers just such facts as Mr. Sumner mentions as 'difficulties and objections,' and the theory should be criticised accordingly.

23. In a footnote regarding my statement about the character of Imperator Mr. Sumner fails to specify what the inference is that the reader 'might easily' make, and so innocent was I of an inference, of which even Mr. Sumner is apparently doubtful, that it was a long time before I could even conjecture what he had imagined. I deny that my statement justifies any inference whatever, easy or difficult, regarding the *reality* of Imperator. We speak of the character and purposes of Caliban, of Faust, of Mephistopheles, etc., without being suspected of dealing with realities. From what I had said over and over again about the personality the inference that 'might easily' have been made is that I was making a statement which was intended to be true on any theory whatsoever, even secondary personality and both conscious and unconscious fraud, and not insinuate an interpretation of which he is apparently doubtful himself. Besides, it would have done no harm to have told the reader that I had said in the very next sentence that the main evidence for my statement was based on evidence not yet published, and then have 'awaited more data.'

24. Mr. Sumner calls my references to the 'serious difficulties in

the way of communication' and to the 'mental condition of the communicator' assumptions, as if to insinuate that they were without evidence. Now 'assumption' is a very equivocal term, doing duty for every imaginable degree of belief between pure imagination and an intuitive certitude. 'Necessary assumption' is an expression we often hear for Kant's 'categories' whose validity seems very little to be questioned. I might show that 'serious difficulties in the way of communication' could be a legitimate *a priori* assumption, but I did not indulge in that sort of science. My statements were based on empirical facts and are true on any theory whatsoever, except conscious fraud. The evidence on which I relied is precisely the same as that which Mr. Sumner accepts in favor of the supernormal and is wholly empirical. Does Mr. Sumner mean to imply that there are no difficulties or mental confusion incident to telepathy and secondary personality? If men would only study the psychology of the record as they are made by their superiors to study Wundt or Kant they might discover that a little knowledge of the subject would go as far as contempt in understanding the problem.

I repeat that I am not defending the theory maintained in my report. That seems to have no importance compared with the problems of reaction time, fatigue, space perception, etc. Nobody, of course, is interested in any questions but these. But I do wish to see a perfectly fair and truthful account of the position which I had taken in my report.

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PSYCHOLOGICAL LITERATURE.

Studies in Auditory and Visual Space Perception. ARTHUR HENRY PIERCE. New York, Longmans, Green and Company. 1901. Pp. vi + 361.

The essays published under this title constitute the first fruits of the Kellogg University Fellowship of Amherst College. The college will be fortunate if the admirable standard established by the present volume is maintained in subsequent publications.

The central problem of the book, which furnishes the principle of unity for a number of otherwise independent inquiries, is the investigation of the actual rôle played by experience in the upbuilding of our visual and auditory space-world. Professor Pierce states frankly that his standpoint is that of a flexible nativism. One may surmise that this view was firmly founded before the author entered upon his experimental work. There is not, however, the slightest evidence that his bias as regards a general space theory has invalidated in any particular the catholicity of his experimental program, or the critical acumen with which he reports upon his facts.

The seven essays dealing with visual space are all devoted to optical illusions. The articulation of this portion of the book is somewhat looser than that of the first half, which consists of an extremely systematic and all but exhaustive study of auditory spatial experiences. So far as the reviewer is aware, there is at present nothing extant which will compare in breadth, lucidity and completeness with this treatment of aural space. An excellent historical résumé of previous theories and observations introduces the report of the author's experiments. These are too extended and intricate to admit of detailed description. They involve, however, tests upon ordinary localization with one, two and three sounds respectively under the most various conditions; tests upon the localizing of sounds in the median plane, upon the effects of the outer ear, upon the idiosyncrasies of intracranial localization, upon monaural localization and upon general auditory orientation, including the probable part played by the eyes and by the semicircular canals respectively. The perception of distance as distinguished from direction is accorded an interesting series of experiments, in which the importance of auditory quality seems quite clearly

made out. The positive doctrine, which issues from all this material, as regards the fundamental principle in ordinary localization of sound, involves the rejection of the several forms of 'semicircular canal theory,' together with the theories involving tactual sensations in the shell and drum of the ear, and the adoption of a view making the distribution of sound intensities to the two ears the matter of primal significance, supplemented at times by the changes in acoustic quality which sounds undergo by reason of the modifying influences of the pinnæ and the bones of the head. The concluding section of this part of the volume reverts to matters opened up in the introduction and presents considerations which, in the author's opinion, constitute a definite support for his nativistic position.

Dr. Pierce regards auditory sensations as unquestionably spatial in intrinsic nature, but he distinguishes sharply between a positional space and a space of extensity, and it is the former variety of spatiality which he believes to be the undeniable possession of sound. The voluminousness of sounds he regards as a characteristic which can neither be proved nor disproved conclusively. The balance of evidence he thinks, however, favors the genuineness of this attribute. As this latter doctrine is somewhat tentatively maintained, we may properly disregard it and dwell a moment upon the claims of the author's positional auditory space.

Let it be said at once, that Dr. Pierce frankly concedes the practical primacy of a space dominated by visual-tactual-motor elements. The localization of sound is consequently a process largely dependent upon the manipulation of these factors. The auditory positional space for which he contends is not, therefore, a thing capable under ordinary conditions of introspective isolation. The one piece of unequivocal (?) empirical evidence for the intrinsic independence of auditory space which the author adduces, is the fact of intracranial localization of sound. As no visual, tactual or motor elements can apparently contribute to this experience, Dr. Pierce concludes that it can only be made intelligible on the basis of localization in purely auditory terms.

It is difficult to follow the author in his assurance as to the conclusive nature of this evidence for an aural space. Of course no question can be raised as to the relatively unimportant part played in these intracranial localizations by vision and the usual forms of tactual-motor experience. For argument's sake one may, indeed, grant that these last-named elements play no part whatever. But surely the facts cited by Dr. Pierce himself regarding the extremely variable nature of these localizations would suggest that some widely variable factors were

responsible for the result and not the mere vagaries of an auditory space sensation, which surely should not be so hopelessly capricious. Now the precise variable desiderated is at hand, if we assume that these localizations are, like so many other extracranial instances, cases in which the position accorded the sound depends upon concomitant sensations from other senses, such as vision. For the mechanical stimulation of the bones of the skull and the sensitive tissues within the cranial cavity is an almost inevitable incident of the conditions producing these intracranial localizations. Whenever such mechanical stimulation of these structures does chance to occur, it may easily furnish a point of reference for the sound to attach itself to. The ease with which sounds located in the median plane may be made to attach themselves to quite erroneous positions tends to confirm this view. The varying density of the bones, the varying modes of conjunction, the differing sizes and shapes of the cavities of the bones, all these and other similar factors are possible sources of the fluctuating nature of these localizations experienced by different individuals.

Dr. Pierce is naturally aware of all this and the disagreement reduces itself largely to a question of the weight to be accorded the several possibilities in the case. Dr. Pierce regards our suggestion as practically irrelevant. Introspection does not clearly reveal anything beyond the auditory sensation. According to well-established psychological doctrine, this could hardly be expected to be otherwise. The two kinds of sensation, if there are two, are always conjoined and cannot, therefore, be readily analyzed. That the sound dominates in the supposititious complex is no more to be marveled at than is the corresponding dominance of taste in certain taste-smell fusions. The interpretation we propose is by no means capable of conclusive demonstration from any adducible evidence. But it is surely a tenable hypothesis and one which is available for those who cannot accede to the doctrine of an auditory space.

Several of the essays on visual illusions have already appeared in substance in the pages of the PSYCHOLOGICAL REVIEW and *Science*. Four of the papers are concerned with illusions of movement and three with illusions of linear direction and contour. Dr. Pierce has been extremely clever in working out experimental conditions for examining the cases with which he deals and many unsupported speculations of other writers are given a *coup de grâce* by his observations. It is of course impossible to bring all his explanatory theorizing under any single rubric. But the position which is most important and most characteristic is found in the rôle assigned to purely

retinal conditions as opposed, for instance, to muscle sensations and the other stock machinery by which visual illusions are commonly explained.

As representatives of this tendency may be mentioned the author's conclusive demonstration that the illusion of the 'kindergarten patterns' is due solely to irradiation, and his persuasive argument that the illusory movements of the columns in the Zöllner diagram are due to a natural interpretation — following our common experiences — of actual movements upon the retina.

The condition of confusion and disagreement among psychologists concerning the whole range of visual illusions is a somewhat scandalous thing at best, and such searching experimental investigations as these of Dr. Pierce are on that account, to mention no others, sincerely welcome. Moreover, the line along which he has chiefly sought his explanations is altogether in accord with the mass of theory in the cognate field of color sensation. It certainly stands for concreteness and simplicity and to the reviewer's mind at least much of it is convincing.

There is necessarily considerable critical comment throughout many of the papers and Heymans, Wundt, Judd, Thiéry and Helmholtz come in for perhaps the lion's share. Two papers, one on the illusory dust-drift seen after the fixation of narrow stripes and the other on two illusions of double movement, contain reports of phenomena which the author appears to be the first to have observed in these forms at least. The first illusion involves an after-image process under the influence of impulses to movement; the second depends upon conditions similar to those of the Münsterberg-Jastrow phenomenon. The explanations advanced are somewhat too tentative and vague for profitable discussion. They are, however, probably all that can safely be said at the present time, a fact that reflects not on Dr. Pierce primarily, but on the prevalent ignorance concerning the causes of optical after-effects from stimulations of movement. The facts observed are extremely interesting and must be reckoned with by any theory which attempts a systematic account of visual processes.

No courageous student of visual illusions can refrain from a tilt at the Poggendorff illusion, whose insidious apparent simplicity has worked havoc with many a trustful theorist. Dr. Pierce is no exception to this well-established tradition. It must, however, be confessed that his careful analyses have enabled him to come off much better than many of his predecessors. He advances seemingly crucial evidence to show that the most important element in the illusion is the tendency

to overestimate the vertical dimensions of the figure. Subordinate to this factor, but exercising distinct and varying influence under various conditions, are to be mentioned the width of the band formed by the parallels, the overestimation of *one* of the acute angles relatively to the other (a fact which seems previously to have escaped notice), and the length of the free ends of the oblique. Evidently we have here a sufficient number of independent variables to account for the most radical divergence of opinion and explanation among observers who fail to treat these elements in an identical manner. The case affords an admirable instance of the futility of supposing that a single principle of explanation will suffice for every illusion which chances to seem simple.

It is to be regretted that the author has not seen fit to add a chapter correlating the results of the experiments on visual illusions with one another and with the discussion of auditory space. Meantime, it is a pleasure to acknowledge the solid merit of the work, which is a thoroughly creditable addition to the experimental psychology of our day.

JAMES ROWLAND ANGELL.

UNIVERSITY OF CHICAGO.

Nouvelles Observations sur un cas de Somnambulisme avec glossolalie. TH. FLOURNOY. Archives de Psychologie de la Suisse Romande, I., no. 2, pp. 102–255, December, 1901.

Professor Flournoy devotes another 150 pages to the case of Mlle. Hélène Smith, the heroine of the volume 'From India to the Planet Mars,' which was issued about two years ago and somewhat later appeared in an English translation (see notice in the PSYCHOLOGICAL REVIEW, July, 1900). It cannot be said that Professor Flournoy has very much new to offer; but there are some points of interest worthy of record. It appears that the fortunes of Mlle. Smith have changed during these two years. Professor Flournoy describes in four stages this change since the publication of the former volume. The first is a stage of irritation and annoyance at the many criticisms to which she as the subject of the volume was subjected. She seemed to feel more deeply than when the several records were submitted to her, the serious difference of opinion between Professor Flournoy and herself, and in a measure to hold him responsible for any adverse or ungallant comments which found their way into the press. In brief, in connection with a growing tendency on the part of Mlle. Smith to believe in the inspired and supranormal nature of her trance revelations, and to feel the sharper contradictions between such a position and that seeking a common-sense explanation, there grew up social and personal compli-

cations which led to Professor Flournoy's degradation to a position of *persona non grata*. In the second phase, Mlle. Smith seems disposed to make amends for this condition of affairs and to recognize that M. Flournoy was really responsible for the celebrity to which she had attained. She accordingly sent him an account of some new revelations so that he might have some interesting developments to present to the approaching Congress of Psychology at Paris. The third stage M. Flournoy calls, though he admits that the designation is 'un peu barbare,' the American stage. It indicates that Mlle. Smith was besieged by a company of zealous admirers all anxious to have sittings, to see the wonders of her trance conditions and to consult her by way of messages from the other world, and so on. The fact that many of these were American ladies is the basis of the designation. Mlle. Smith throve in the sympathetic atmosphere thus provided, and her automatic life continued to blossom and send forth new branches. By the munificence of a lady of wealth, Mlle. Smith was relieved of the uncertainties of earning a living as an employee of a shop at Geneva, and was thus enabled to devote herself more uninterruptedly to her psychic experiences. In the present stage Mlle. Smith has been visiting in Paris, has gathered about her a more or less sympathetic company of believers in her spiritualistic and supranormal powers, and as a consequence much of Professor Flournoy's information is at second hand, or in the form of letters. Yet he is not altogether banished from the medium's favor, though he must be content to receive and not to direct the current of inquiry. It is hinted, too, that Mlle. Smith is herself going to turn author and publish her own spiritualistic account of her story, using the material which she refuses to place at the disposal of M. Flournoy.

Following the Martian revelations which were detailed in the former volume, the medium removed the scene of her visions to a further planet, which is here designated as ultra-Mars; and here again we have descriptions of scenes and peoples, occasional sketches, a new language of strange sounds though of French structure, as was the Martian tongue. Pages of the texts are given, as are also samples of the scenery and interiors of the ultra-Martians; and they are more or less childish in their attempts to suggest the bizarre and unearthly. Worthy of note are the ultra-Martian ideograms or set of symbols by which the words are presented. These are about as different and as perplexing as the characters of Chinese; and the fact that they are consistently employed constitutes a notable feat of the subconscious memory of Mlle. Smith, and the more so since this is done for more

than one of these fictitious languages. But there are further worlds beyond Mars; and the next trance cycle takes place on Uranus, from which in turn the scene shifts to the moon. Again a curious geometrical alphabet, different from the others, and yet for brief passages consistently maintained; again descriptions of strange scenes and the usual accompaniments of her remarkable imagination. The evidence thus furnished of the continued growth of these somnambulic creations is indeed noteworthy, and gives a more profound sense of admiration for the subconscious mentality than one is likely to have held before. Still more important is it to note that the genesis of these cycles follows one genetic course. They all begin, so far as the linguistic part is concerned, with the subject's hearing a few words of the unknown tongue, and later on repeating them; still later she sees the characters, and finally is able to reduce them to paper by automatic writing. Leopold, her spirit guide, takes a variable part in the translation. There are always considerable intervals between the stages of such a cycle; these M. Flournoy calls periods of incubation or subconscious preparation. The languages and the celestial scenery are thus not a sudden revelation, but come only when the subconcious attention has been directed to them for a relatively long time. There are likewise further revelations of the Hindu cycle, in which the medium had produced a language with recognizable Sanscrit elements, and had become the incarnation of a former Indian princess. The Sanscritoid character of her linguistic invention is further established; likewise the complete historic anachronism between this speech and the character which the medium is supposed to be personating, for the latter in real life knew no Sanscrit. The origin of the information which Mlle. Smith possesses in this cycle is not clearly set forth, but enough is shown to make it evident that a retentive memory would have found the data in various incidents accessible to her reading and experience.

The royal cycle in which the medium becomes Marie Antoinette is likewise not neglected, and is naturally aroused by her visit to Paris, where indeed she enacts some of her trance scenes amid the original settings of the unfortunate queen. Here too, a historical character is introduced with very unhistoric concomitants, Dr. Barthez, a physician of the court of that period.

There are further evidences of the continued activity of the subconscious and its connection with the emotional life of Mlle. Smith. The efflorescence of the Marie Antoinette cycle under the inspiration of a visit to Paris is a case in point. The fact that upon learning the news of the fortune presented to her by her benefactress she experienced a

vision of Leopold, is another. She was about to step on a tram-car to bring the good news to her mother when Leopold appeared on the step of the car, forced her back, and conducted her through the crowded streets to the shop of her employer, whom she then bade farewell. It is also interesting to note that this subconscious faculty may be put to some use. On one occasion Mlle. Smith thus recovered a piece of goods which she had mislaid or had witnessed some one else lay aside; and on another occasion she recovered a similar lapse of memory by an appeal to Leopold.

Perhaps the most striking part of this sequel is that it differs so little from the former chapters of the detailed story. The story still goes on, and in its continuance emphasizes the correctness of Professor Flournoy's diagnosis. The source of all this peculiar intellectual activity is the subconscious romancing imagination of Mlle. Smith. No other hypothesis is in the least adequate; and no other receives the successive corroboration which this one receives from the anachronisms and the details of the various revelations. The whole remains a noteworthy case of 'mediumistic' phenomena, and will serve for the edification and furtherance of, and not as an impediment to, the psychology of these phases of mental expression in so far as their connection with the normal mental life is appreciated and established.

JOSEPH JASTROW.

Ueber Begleiterscheinungen psychischer Vorgänge in Athem und Puls. P. ZONEFF und E. MEUMANN. Philosophische Studien, Vol. 18. Pp. 1–113.

This investigation divides itself into two parts. The first is a study of the relation between the various forms of attention and the changes in respiration and heart rate, the other of the effects of pleasure and pain upon these physiological processes. The records were taken by means of the sphygmograph and pneumograph of Marey. Two pneumographs were used, one was fastened about the body between the first and second ribs, the other below the sternum, and each was attached to a framework of cloth in a way which permitted successive experiments to be made with the same adjustments. It was thus possible to compare the effect of the various stimulations upon the thoracic and abdominal breathing.

The forms of attention were so chosen that they covered the different senses. Among the tests were reading, counting the crosses upon a line, hearing out overtones, obtaining the least noticeable pressure by means of von Frey's hairs, and mental calculations. The ac-

companying effect upon the respiration was an inhibition. In the milder forms of attention this showed itself in the quickness and shallowness of the respiratory movements, while in the more profound forms there was almost a complete cessation of the movements. The intellectual was distinguished from the sensory attention by its more pronounced effects. The pulse was uniformly slowed during the attention process.

Pleasure and pain were induced by colors from gelatine plates, by consonant and dissonant tones, by tastes, and by memory images both simple and complex. The investigators confirmed the classical statement that the effects of pleasure and pain upon both breathing and pulse were exactly opposed. The respiration was quickened and more shallow, the pulse slowed, during pleasure, while respiration was slow and deep in pain, with quickened pulse. The thoracic breathing was always most affected, and the abdominal was either not changed at all or showed a change in the opposed direction from the thoracic.

In addition to repeating more carefully these fundamental experiments, the authors obtained interesting results upon several related problems. The first was as to the relation between the attention efficiency and the corresponding physiological processes. Records were taken while the reagent was endeavoring to hold a copper wire just above a metallic plate. It was found that the greatest number of contacts were made at the time when the bodily processes were least characteristic of active attention. It is suggested that this indicates that the fluctuations of the attention may be traced to its physiological side. There is not mentioned the probability that both the changes in pulse and respiration rate and the attention fluctuations may be due to the rhythm of the vaso-motor center.

Another statement of the text-books, founded upon self-observation, which was investigated was the effect of directing the attention to the feeling process itself. The usual statement that attention to the feeling destroys the feeling was not found to be true of the effect upon the physiological concomitants. On the contrary, the effects upon both respiration and pulse are increased. The authors however state that they think the result would have been different if psychological introspection had been attempted instead of mere attention to the feeling.

Distraction of the attention destroys the effect of the feeling, whether of pleasure or of pain. The result is always the characteristic of the attention alone.

The experiments as a whole seem conclusive with regard to the opposed effects of pleasure and of pain on respiration and pulse. It is much to be desired that some one carry on a similar investigation upon the effects on body volume. W. B. PILLSBURY.

UNIVERSITY OF MICHIGAN.

An Analytic Study of the Memory Image and the Process of Judgment in the Discrimination of Clangs and Tones. G. M. WHIPPLE. American Journal of Psychology, Vol. XII., No. 4, 1901, pp. 409–457.

The aim of this investigation was chiefly to determine how much influence the memory image of a tone has upon its discrimination from another tone. Reed tones as well as bottle tones were used. Three of the six observers were musical, three unmusical. The reed tones used as standard tones were of 612, 724, 832, 928, and 984 vibrations, the variations about 8 vibrations. The observations recorded are highly interesting from more than one point of view. Only a few can here be mentioned. Increase of the time interval caused a general decrease in the total number of right cases. This, however, can not be explained by corresponding changes which the memory image may be supposed to undergo. On the contrary, the memory image was often found by introspection to be exceedingly weak or entirely lost when the judgment was certain and correct. Occasionally a good image may be present without insuring a decision. Sometimes reports were given like this: *V* was way below my image, yet I felt compelled to judge 'equal' on account of some feeling of familiarity. The practice effect is almost entirely confined to the unmusical observers. Within the octave employed there is no observable dependence upon pitch. Writing down the judgment, or repeating the tonal interval several times, often brings on a feeling of certainty which was not present when the judgment was made. The unmusical observers often judged 'different' without deciding whether lower or higher. The musical observers almost never confused the direction of a difference. In one case light was thrown upon the mechanism of judgment as conditioned by what might be called ease of reproduction: "I knew the difference easily, but not the direction; said lower because it is easier to say." If *V* differs from *N*, the process is not, as a rule, felt to be so largely auditory; the attention is entirely taken by a complex something which stands for high or low. What the something is depends upon the individual; its core is usually a complex of strain sensations, its remoter elements visual or

organic. If *V* fails to engender either the reaction of familiarity or of specific difference, the observer resorts to auditory comparison. The resulting decision is usually uncertain and very apt to be incorrect. Pleasantness is the correlate of 'certain' judgments, not of any of the categories equal, higher, or lower. The verbal formulation arises only after the decision has been made otherwise. Speed, certainty, and correctness of judgment are closely correlated. In experiments with distraction during the time interval by means of odors, it was found that distraction slightly lessens the total number of right cases for all observers, though, it may be added, this effect is not due so much to the loss or impairment of the image as to the time consumed by the shift of 'venue' at the moment *V* sounds; *V* fails to 'sink in' at first if the distraction be complete. MAX MEYER.

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SOUL PROCESSES.

Psychische Vorgänge und psychische Causalität. TH. LIPPS. Zeitschrift für Psychologie und Physiologie der Sinnesorgane, Vol. 25, pp. 161–203.

This absorbingly interesting paper is occupied with the proof and elaboration of the two following theses: (1) Conscious states can only be related to one another indirectly or through their connection with the unconscious soul-processes (*psychische Vorgänge*) in which we must assume them to be grounded. (2) Conscious states as such are absolutely passive and have no power to modify the working of their unconscious substrates.

Professor Lipps believes that the aim of psychology should consist not merely in describing mental phenomena but in making their relations intelligible. Just as the physicist in order to reduce physical facts to causal order finds it necessary to supplement the objects of immediate experience with conceptions of ether and atoms, so also the psychologist is forced to infer, as the direct basis of the fragmentary series of subjective conscious contents, a continuously existing order of real things, that is, things which can be objects of thought but which in no way depend for their existence upon our being aware of them. We must, for example, posit a soul, first, as the substrate of the immediately felt subjective unity or I aspect of each psychosis, second, as the bearer of the system of thought-traces and dispositions which are bound up with the I. And the same reasons which make it necessary to infer the existence of a soul as the substrate of the unity of our conscious life, make necessary the further assumption of real

processes or occurrences in the soul as the substrates of the specific and relatively differentiated states of consciousness. Professor Lipps does not undertake a detailed classification of these soul processes or *psychische Vorgänge*, but from what he does say we gather that he regards each form of *sensation* as grounded in a distinct soul process, while *representations* (*Vorstellungen*) may be adequately described as grounded in complexes of the sensation-substrates. As for the rest of our conscious states, particularly the 'feelings' of time and space relations and of similarity and contrast, they are to be thought of somewhat in the Herbartian manner as resulting from the dynamical relations of the sensation-substrates to one another and to the soul in which they occur.

The strongest reason for believing in these *psychische Vorgänge* is that without them it is impossible to speak of psychical causality—or, indeed, of any other real relation between conscious states. For suppose we try to disregard the real grounds of conscious states and think simply of what we are conscious of. A conscious content regarded merely as such is purely ideal and can have only ideal relations to other contents. Someone may think of one content as causing another, but, needless to say, it is not people's thoughts of psychical causality but psychical causality itself that the psychologist seeks to determine. And in the same way with the pure time relation: a conscious content *as such* cannot precede or follow anything, for as a mere ideal content it has no place in a real time series. In short, Professor Lipps seems to feel that a content considered as such, that is, as purely ideal, cannot stand in any real relation, for the same reason that an ideal dollar cannot pay a real debt. If conscious states are to stand in any real relation, it cannot be due to any peculiarity of content. To what, then, can it be due? Obviously, to the only thing that we can connect with a conscious state except its content, *viz.*, its source or ground in a soul process. Not *what* we are conscious of, but *that* we are conscious of it, makes it possible to speak of a conscious state as standing in real relations.

On this very subtle and, as it seems to me, for the most part convincing argument Professor Lipps bases his plea for the *psychische Vorgänge*. He then proceeds to show that his view is in harmony with the naïve realism of common sense, and also to some extent with the attitude of the materialistic physiologist. For the 'plain man,' if asked whether the qualitative content *a* calls up the second content *b*, will reply that it is rather his *perception* of *a* which calls up his perception of *b*. As for the physiologist, he too will insist on the fact that the

perceived sequences of consciousness do not explain themselves but are explicable only by assuming an interconnected system of real processes underlying them. But because we can agree with the physiologist thus far, we are not obliged to concur in his further assumption that the direct grounds of conscious states are processes in the brain. For "the processes which are started by an external stimulus tend to bring into existence a sensation-content, and on their way to this goal they pass through a sphere in which they are brought into interaction with one another and also with concurrent processes which have originated in that sphere without the agency of an external stimulus. This sphere is distinct from the sphere of conscious life; it is the sphere of the real psychical occurrence. 'In the soul' [*Psyche*] means 'In this sphere'" (page 180). We must, moreover, be careful to note that the *psychischer Vorgang* never *becomes* a conscious state, it only tends to introduce one. When the *Vorgang* attains a sufficient degree of intensity, a sensation content '*steps in*';—just how or why this happens is the great riddle.

Having thus established the existence of the *psychische Vorgänge*, Professor Lipps passes to the justification of what we have called his second thesis. Consciousness has no effect upon the *psychische Vorgänge*. The unconscious soul-life with its complicated interactions and transformations is no more affected by the conscious contents which a portion of it occasionally brings about, than the waterfall is affected by the rainbow that plays now and then over its surface.

The arguments offered by Professor Lipps in support of the 'epiphenomenality' of consciousness are both general and special. The general argument is most explicitly stated on pages 171 and 185. It consists in the assertion that real objects and real relations are never matters of immediate experience but are entirely the result of inference; for the only thing presented in immediate experience is the purely ideal content, which has, as such, no place in a real temporal or causal order, and is hence necessarily without influence on the *psychische Vorgänge*. Professor Lipps, if I have caught his meaning, regards the truth of this statement as the self-evident result of introspection. Look into your mind and you will see there nothing but ideal contents. The 'real' is a pure inference and as such is always unconscious. We venture to think that this would not be accepted by most people as an adequate account of what they immediately experience. Introspection shows us not merely ideal contents in purely ideal relations but ourselves actually perceiving those contents. *That* we perceive is no less truly a datum, than *what* we perceive.

It is, indeed, necessary to acknowledge that we have an immediate consciousness of the real, if we are to believe in a real world at all, for, as Professor Case has so forcibly shown in his book entitled 'Physical Realism,' it would be quite impossible to frame by mediate inference a conception of something of whose nature we had had no inkling in direct perception. We could no more conceive of reality if we had not felt it 'up against' us in every sensation, than a man born blind could conceive of color.

But perhaps an even more serious objection to Professor Lipps's general argument for epiphenomenalism is its apparent inconsistency with his belief in the power of real processes to bring about conscious states. If there is sufficient homogeneity between the real and the ideal to admit of the *psychische Vorgänge* causing the conscious contents to 'step in,' then it seems hardly justifiable to deny that that same 'stepping in' or becoming conscious could react upon the unconscious conditions which produce and sustain it. If one-way action is a fact, interaction is at least conceivable.

Professor Lipps is, however, by no means content with urging general considerations in support of his second thesis; he examines the phenomena of attention, association and analysis (where, if anywhere, we should expect to find mental processes influenced by conscious contents), and undertakes to show that in every case it is the unconscious process which guides and conditions the conscious without being in any way reacted upon. In this part of his paper Professor Lipps certainly gives us a remarkable demonstration of the utility of his theory in throwing light upon the more obscure phases of attention and association. One feels that the author's ideal of making psychology an explanatory as well as a descriptive science is by no means impossible of realization, and that the *psychische Vorgänge* have a right to exist, if not as demonstrated verities, at least as a very valuable working hypothesis. When we consider, however, that the nominal purpose of this portion of the essay is the disproof of the efficacy of the conscious and only indirectly the justification of the unconscious, we are inclined to feel that the special arguments to this end are as unconvincing as were the general. In the treatment of attention, for example, we are made to see very clearly that the content to which we attend can not draw our attention merely in virtue of its own ideal nature. The 'thought-traces' and dispositions which are the soul's record of past experience are acting upon one another in such wise as to make possible the attention to the present content; but granting that the chief determinants of our acts of conscious attention are the

unconscious inhibitions which the *psychische Vorgänge* exert upon one another, it by no means follows from this that the nature of the conscious content may not also affect the attention. Again, in the case of association, Professor Lipps succeeds in proving that the associative tie between similar contents is due not merely to their similarity but to the fact that the soul processes which underlie them are linked together. But he does not, as it seems to me, prove the further and much more radical thesis, that the feeling of similarity, or the unconscious basis of that feeling, is entirely uninfluenced by the actual similarity of the contents. Is not the fact that we perceive two triangles to be more similar than a triangle and a circle due in any degree to the fact that as conscious contents they actually are more similar in so far as they contain a greater number of identical qualities? Or must we hold to the paradox that the similarity which we think we perceive between conscious contents is a pure delusion due entirely to the unconscious *Vorgänge?* And, thirdly, in the case of analysis, Professor Lipps, if I understand him, attempts with equal unsuccess to prove that when we think we perceive and analyze a complex conscious content, we are really perceiving a content that is quite simple, the delusive feeling of complexity being brought about by the interactions of the several sensation substrates which combine to occasion the representation-content.

Underlying the arguments both general and special which Professor Lipps urges in support of his second thesis, is the strong conviction on his part that it follows as a corollary from the first thesis. The efficacy of the unconscious *means* the inefficacy of the conscious. Now if we think for a moment of the grounds which justify the first thesis, I believe we shall find that it is the falsity of the second thesis rather than its truth which is implied. The real basis for assuming a soul and soul processes is after all simply one case of the need of postulating *continuity* between the disparate members of a single system. The motions of the brain molecules are certainly bound up in one system with the intermittent series of conscious states which make up our experience, while it is equally certain that they are disparate. Two courses seem open. We may, with the materialist, regard the permanent ground of our recurring experiences as the brain, and define thought as a mode of motion. Or, on the other hand, we may, with the transcendentalist, take the timeless ego or unifying principle of consciousness as the real self and define brain motions as a mode of thought. The objection to both theories lies in the fact that consciousness and motion in space are so disparate that neither can be properly

conceived as a mode of the other. We cannot help matters by listening to Hegelisers who would deftly explain the relation of thought and brain motion by the platitude that they are 'abstract aspects of a concrete organic unity,' for it is precisely the fact of an organic unity of disparates that calls for explanation, and the statement of the problem can scarcely be accepted as its solution. Now when confronted with this sort of problem in the past, science has always adopted one course; knowing that continuity must exist between two related elements, and not being able on account of their disparateness to express one as a mode of the other, it has posited a *tertium quid* as homogeneous with both elements and hence the basis of their continuity. The soul with its processes or *Vorgänge* is thus to be posited as the medium by which brain motions occasion the consciousness which reflects them. If this is at all a fair statement of the matter, it would seem clear that Professor Lipps is directly at war with himself when, after positing as the basis of the conscious *Ich-gefühl* a similarly simple real soul whose *Vorgänge* are continuous with physiological stimuli, he proceeds to deny that these intermediary processes are in any degree homogeneous with, or modifiable by the conscious contents which they are admitted to occasion.

But Professor Lipps's article is interesting not so much for its possible failure to prove the epiphenomenality of consciousness, as for its success in proving the *psychische Vorgänge* and in applying them to special cases of psychical causality. His hypothesis seems, as we have already said, to be a very real step toward making psychology an explanatory science by raising its empirical laws to that plane of *Anschaulichkeit* or quantitative continuity which according to Kant is the goal at which every science should aim. To this goal the physical sciences are fast attaining, and, if we may trust Professor Lipps, there is no reason why psychology alone should continue to be burdened with the categories and methods of obscurantism. The paper will be welcomed by all who sympathize with the Kantian ideal of scientific method and with Herbart's fine confidence in the applicability of that ideal to the phenomena of mind.

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A History of Modern Philosophy. DR. HARALD HÖFFDING, Professor at the University of Copenhagen; translated from the German edition by B. E. MEYER. London, Macmillan and Co. 1900.

There are several points of view from which a review of such a work as this may be written. The critic may regard it as his func-

tion to indicate the completeness or incompleteness with which the author has succeeded in gathering up and displaying the philosophical opinions of the greater and the lesser lights that have an admitted right to be enumerated among the philosophers; he may dwell upon the skill, or the lack of it, with which the genesis of successive systems of thought has been traced, and the sense of proportion which is exhibited in the space allotted to the discussion of schools, of individual men, and of opinions; he may criticise the bulk and the exactitude of the author's information, the thoroughness of his acquaintance with the original writings which constitute the true history of speculative thought, and the keenness of his insight into the real meaning of passages whose obscurity has been a stone of stumbling to many generations of students; he may point out the originality with which the matter which is presented has been set forth, and may congratulate the author upon his gift of literary expression, not forgetting to abuse the translator for this or that blunder in the rendering of passages which have lost their true character in putting on another dress.

But Professor Höffding's excellent work has been for some little time before the public. It has received a good deal of criticism of this sort, and it is not my intention to add to it. I wish to dwell rather upon the purpose which he has held before him in the production of these volumes, and to indicate the significance of his method for the study of philosophy, not as literature or biography, but rather as science—science in the broad sense of the word, which does not limit its use as it is too often limited by the unthinking.

Dr. Höffding points out with justice that it is wise to devote much attention to the history of philosophy. In this field the past cannot be ignored as it seems possible to ignore it when one is cultivating many scientific disciplines. There is no established body of doctrine which we have a right to impose upon the student as authoritative. He who would attain to a just estimate of the problems of philosophy as they present themselves to our age must approach them with a mind enriched by a knowledge of the forms which they have taken in the past, the shapes which have been given to them as a result of the labors of the great reflective minds of the race. Only then can he hope to distinguish between the accidental and fleeting, and the relatively permanent; between the passing prejudice or misconception of a day, and that which may turn out to be a true insight into the nature of things and worthy of being regarded as a contribution to scientific truth. Such historical studies reveal that several factors influence the treatment of the problems of philosophy. Dr. Höffding recognizes the following:

1. The personality of the philosopher. As we are not in the realm of exact knowledge, we may expect the personal equation to be large, and, in fact, we do find that it is very large.

2. The observations which are taken as a basis for the construction of a philosophical system. For example, the development of the natural sciences has been of the greatest importance for modern philosophy. Its leading problems seem to be determined by the fact that these sciences have arisen. Historical circumstances, and intellectual movements of many sorts, may be seen to be responsible for the turn which philosophic thought has taken at many a critical point in its history.

3. The consistency with which initial assumptions are laid down and maintained, and their logical consequences deduced from them.

The exposition and criticism of the systems treated of in the two volumes before us turn upon these three points, but these points are not given an equal amount of attention. The author attaches most weight to the first two. He thinks that inconsistency in a great thinker 'is often nothing but the natural consequence of the fact that his genius displays itself in *several* lines of thought, although he may not himself have been able to follow these out far enough to discover their mutual contradiction' (Introduction, p. xvi). He regards it of importance that each of these lines of thought should be developed, while freely admitting that it is desirable that depth of thought and consistency should be united.

That the position taken by Dr. Höffding in his introduction is not belied in the body of his work, a careful examination of his volumes will reveal. It should be observed that this makes his book rather a history of human culture, presented in the form of a series of spiritual biographies, than a critical history of philosophy written with a strictly scientific aim. I do not, of course, mean to maintain that a recognition of the personal equation may not be of value in philosophy as elsewhere, nor that a knowledge of the historic background of a system may not help us to understand the real significance to the writer of statements which, to a man in a different intellectual environment, must remain obscure. But I do mean to point out that it is one thing to discover how a man came by a thought, and quite another to estimate its value as a contribution to human knowledge. If, in writing a history of philosophy, we devote our attention largely to the former, our work is anthropological, historical, biographical, but it is scarcely philosophic. If we take philosophy seriously, and make it our first aim to attain to a clear and unprejudiced view of the problems with

which it concerns itself, we shall not overlook the personal equation and the historic setting of a philosophical system, but we shall regard any attention paid to these as merely preliminary, as a something wholly subsidiary to the most important part of our investigation. The problems presented to reflective thought are not new. There has been no such general and indubitable progress in reflection that we are justified in approaching the philosophy of a Plato or an Aristotle as merely matter for interesting historical investigation. The absorbing question is whether the initial assumptions which it seemed to such a man reasonable to lay down may be accepted, in part at least, by ourselves. In weighing such assumptions, we must be influenced, not merely by their seeming agreement or disagreement with the facts of experience as we see them, for we, as well as those who have preceded us, may misinterpret our experience; we must be influenced also by a realization of the consequences which seem to follow from such assumptions, and we may find that the men of the past have traced such consequences with great labor and ingenuity. By this labor and ingenuity we may profit; and he has not labored in vain who has shown that a road which many may be tempted to travel, leads to no desirable goal or ends in an *impasse*.

I feel, therefore, justified in entering a friendly protest against the conception of the history of philosophy embodied in these volumes. It concerns itself, it is true, with what men have thought and how they have come to think it; but it does not sufficiently recognize the great importance of the distinction between coming to think a thing for no better reason than that it appeals to one's psychological idiosyncrasies, or is suggested by this or that book or by this or that social or religious movement, and coming to think it because one's genius for reflection enables one to see with clearer vision than is granted to most men the problems which human experience lays before us, and the possible solutions which may be found for them. Moreover, it should not be forgotten that the full meaning of a conception as it presented itself to a philosopher of constructive genius may be very inadequately portrayed if we depend largely upon an indication of the sources from which the man has borrowed, and overlook the fact that it may mean to him much more than it did to those from whom he has taken it. What it really meant to him can only be determined by the most careful examination of its place in the man's own system of thought. The fact that other men have had a more or less similar conception, and have used the same word to represent it, may be positively misleading. Neither Aristotle, nor Augustine, nor Thomas, nor Bruno, nor

Spinoza, nor Kant, coined for himself, by a free act of intellectual creation, the conception of God. Each appropriated something that he found ready to hand, and he appropriated also a word in common use. Is it possible to attain to a just estimate of what the word meant to any one of these thinkers if we pay attention chiefly to his psychological peculiarities and to the materials which he inherited and with which he was compelled to begin his construction? Must we not come back to his own system, and strive to obtain a comprehension of its real structure? In other words, must we not dwell upon his initial assumptions and the consequences which he found it possible to draw from them? If we assume that every time a philosopher uses the word God he means what was meant by those who preceded him when they employed the same word, we are in danger of falling into an error analogous, at least, to that into which we should fall did we assume him to mean what we mean when we use the word.

The defect of method upon which I have dwelt in the preceding pages does not, of course, make itself equally apparent in all parts of Professor Höffding's work. There have been ages in which men have borrowed largely from the past and have not greatly modified the content of that which they have borrowed. There have been times when the broadening and deepening of existing conceptions have seemed to be the inevitable result of a rapid development in the field of natural science. There have been eclectics whose opinions can be well understood when we take into consideration the two factors upon which Dr. Höffding mainly dwells. Any man can read with profit his fine chapters upon 'The Renaissance and the Middle Ages' and upon 'Humanism.' Nor can one complain of his treatment of such a writer as Montaigne, who was not a man of constructive genius, nor one whose opinions form a whole each part of which loses much of its significance when divorced from the rest. But it is a different matter when Professor Höffding treats of such a thinker as Bruno.

When one has pointed out the profound significance of an abandonment of the mediæval homocentric world-scheme and of the adoption of the Copernican system with what seem to be its implications and suggestions; when one has dwelt upon the antagonism of the newly-liberated mind to the Aristotelian doctrine that had long held it in bondage; when one has traced certain conceptions and forms of expression to the influence of the Platonic doctrine, or to the speculations of Cusanus or Telesio; it does not follow that one has made plain to the reader the actual course of Bruno's argument in his most

important work, the dialogues 'Della Causa, Principio ed Uno.' Here Bruno has an argument; he does not merely express opinions. He argues loosely, he more than once changes his point of view, he depends upon quibbles which cannot easily be understood by one who is not familiar with the scholastic distinctions against which Bruno rebels; but he always argues, and it is perfectly possible for the patient student to attain to a comprehension of the transitions in his thought. There is, however, no other way of attaining to this than by a careful examination of his processes of reasoning. It is of little value to sum up his conclusions in general statements; to tell us that 'at every single place, in every single particle, the world-soul works as a totality.' A man may hold this perfectly well without being a Bruno at all, and without even comprehending Bruno's argument to show *how* such a thing can be possible, and what can be meant by this dynamic ubiquity. One may read and reread Dr. Höffding's discussion of Bruno's 'Fundamental Philosophical Ideas' (Vol. I., pp. 130–139), and yet obtain no clue to the rhapsodic utterances which compose the latter part of the third dialogue. Yet he who does not possess such a clue does not know Bruno. He is in danger of making him too modern a man; that is, he is in danger of making him what he was not.

A similar criticism may be made touching the treatment accorded to Spinoza. I know of no book likely to be studied by those who occupy themselves with the history of speculative thought which presents more difficulties than the 'Ethics.' To most readers it is practically a sealed book, so far as the actual structure of Spinoza's thought—the true course of his argument—is concerned. They gather from it certain luminous ideas, and they certainly read it not without profit, but they rise from its perusal with the vaguest possible notion of the bony structure, if I may so speak, of Spinozism. There are many pages in which the course of the argument is not apparent, and which they have followed with uncomprehending eyes. Professor Höffding's exposition of the 'Fundamental Concepts of the System' (Vol. I., pp. 307–314) will not give such pages a meaning, nor will any account of the life of Spinoza or of the sources of Spinozism take the place of a clear and systematic exposition of the course of the reasoning which unfolds itself in the 'Ethics.' I ask anyone who doubts this statement to see for himself whether anything that Höffding says will afford a clue to the meaning of such passages as the following: 'Ethics,' I., 5; Epistola, L.; 'Ethics,' I., 8, schol. 2; 'Ethics,' I., 13, and I., 15, schol. And yet the comprehension of these passages and

such as these is of the utmost importance to the comprehension of the conception of immanence and its place in Spinozism, which is much the same as saying that it is of the utmost importance to the comprehension of Spinozism itself.

I have chosen to speak of Bruno and Spinoza because it is in treating of such thinkers as these that Professor Höffding's method of writing the history of philosophy seems to be at its worst. I have felt free to dilate upon what I regard as a defect, and to pass over many excellencies, because the merit of the work is incontestible, and is generally admitted. Within the limitations made necessary by his method, Professor Höffding has written an excellent book, learned, clear, interesting and sympathetic. I have read it, and I shall again read it, with pleasure. But one may say all this and still hold that it is a book of one class rather than of another—a book for the general reader who wishes to know in a general way the opinions which men have held and to understand how they could come to hold them, rather than a book for the serious student of philosophy, to whom biography and history are, after all, matters of minor importance, though they are matters which the accomplishment of his purpose makes it impossible for him to leave out of account. There can be no doubt that the book is the more readable for being what it is, and that it will appeal to a larger circle than it would were it written upon a different plan. For one thing, the somewhat superficial treatment accorded to the doctrines of many philosophers makes it possible to mention, at least, a large number of men who would have to be omitted altogether in a work no larger than this one which attempted to treat philosophical systems in a thorough and analytic way. Of such men one likes to know something, and one is apt to feel defrauded when they are passed by in silence. Professor Höffding's work fills a place, and an important place, among books which treat of the *placita philosophorum.* I sincerely hope that many will read him, and that the *leves gustus in philosophia*, which he affords the reader, will lead some on to the *pleniores haustus* which must be sought for elsewhere.

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Ethics Descriptive and Explanatory. S. E. MEZES. New York and London, The Macmillan Company. Pp. xxi+445.

Professor Mezes' work is, I think, the most important, if not the first, systematic 'attempt to construct a positive or purely scientific theory of ethics, and to give a naturalistic account of all the aspects

of morality and immorality' which has been produced in this country. His sane and modest preface is well calculated to ensure the sympathetic attention of readers who diverge most widely from some of the positions which he maintains. He conciliates metaphysical moralists at the outset by the statement that while undertaking to write a scientific account of ethics, he does not undervalue or fail to realize the need of a metaphysic of ethics. There are in fact, he says, two distinct questions, viz.: What is morality? and, What is the cosmic significance of morality? The former question, to which Mr. Mezes confines his attention, 'can be answered quite as scientifically as the question, What is a living being?' The latter question is left for metaphysicians to handle; and there can be no serious objection to this division of labor. It is a somewhat unusual merit for 'scientific' moralists to recognize that there are these two distinct questions, and that the answer to the former of them does not of itself furnish an answer to the latter also.

Nevertheless, Mr. Mezes, who has a perfect right to set the boundaries of the subject he wishes to treat, seems to me to take too narrow a view of the scope of ethics. "Ethical writers," he says, "do not in any proper sense judge conduct or issue pronouncements as to what is right or wrong. Their more modest task is to discover and record men's genuine judgments as to what is right and wrong, or in other words, to discover what men really mean by right and wrong." "In an ethical treatise, all statements of what is right, as distinguished from statements of what men hold to be right, are open to distrust" (p. 7). Thus in spite of the fact that the author regards ethics as a normative science, he would really seem to limit it to the function of description—only, instead of describing 'conduct actually performed,' it is the function of ethics to describe 'an ideal type of conduct, actually so conceived by men,' or in other words, to state 'what men hold to be right.' But a science which simply records what men think, is just as much descriptive and just as little truly normative as one which records what men do. No doubt the knowledge of what kinds of conduct men are fairly agreed in regarding as right, will help the moralist towards an adequate conception of the norm, conformity to which constitutes right conduct. But the mass of mankind's intuitive ideas of right and wrong conduct are formed without any conscious reference to the ideal or normative end. And ever since the time of Aristotle a long line of representative moralists have held that it is the chief function of ethics, by reflection upon the nature of man, to determine the supreme end or good of human life. Mr. Mezes' objection

to defining morality in terms of an ideal end to be realized, or, as he puts it, the objection to designating ethics a 'teleological science,' is that this would be to assume that "it is bound to describe moral actions in terms of the result or end that they realize"; but since Kant and others hold otherwise, such an assumption is not warranted at the outset, however true it may prove in the sequel (p. 18). This caution, based upon the desire to give a definition which is broad enough to include all types of ethical theory, and not to anticipate results in such a way as to limit the scope and method of the science unduly, is commendable.

But there is some danger, on the other hand, of too narrow a conception of the function of ethics as a normative science. To say that ethics as a normative science is called upon to describe the standard actions that deserve to be called moral (p. 8), is not precisely the same thing as to limit it to the rôle of describing the actions which happen to be so called. Conduct may be held to be right for one of two reasons: either it is right in itself, without reference to the end to be attained, or it is right according as it furthers the Supreme End. If the former be true, then, in so far as men *know* what is right, a statement of what they hold to be right would at the same time furnish the norm of conduct. Thus ethics as a descriptive or historical science would at the same time be normative. But men admittedly are often mistaken as to what is right. While if right conduct be defined with reference to some supreme end, and men are not always agreed as to the nature of this end, and are often mistaken as to the best means to its attainment, *i. e.*, do not know what is right (and Professor Mezes emphasizes the wide divergence of opinion as to what is right), then the record of men's opinions as to what is right conduct, even if this could be said to constitute a normative science, would not, in my opinion, exhaust the scope of ethics. The adjective 'normative,' as currently used, is a question-begging word to which it is not well to attach too much significance. The important question is in regard to the nature of the problem with which ethics deals, and the reviewer differs from the author in holding that ethics does establish a norm or standard and judge or evaluate with reference to it, and does not simply record men's judgments as to what is right or wrong (7).

The first problem of ethics is to determine the norm, by which we mean not simply the description of a moral standard 'actually so conceived by men,' but a determination of the validity of the ideal; and the second problem is to determine, by reference to this norm,

'what right and wrong, morality and immorality respectively are.' In short, we wish to know not simply what men hold to be right, but what *is* right; and if ethics is incapable of satisfying this demand, there should be some other science—say metaphysics—whose function it would be to deal with the validity of the moral ideal and the reality of the distinction between right and wrong which is implied in our common moral judgments. In that case ethics would not only not be an independent science; it would derive its strictly normative character from its alliance with metaphysics. If, 'in an ethical treatise all statements of what is right, as distinguished from what men hold to be right, are open to distrust,' we must either look elsewhere than to ethics for an answer to the ethical problem, or we must admit that there is no answer. Meanwhile, a very respectable body of thinkers have always held that it is the business of ethics to deal with this very problem, and until some science shall by mutual consent or *vi et armis* appropriate this territory, ethics is not called upon to surrender its prescriptive rights.

Nor, as a matter of fact, does Professor Mezes ignore the deeper ethical questions so entirely as his definition of the scope of ethics might lead one to suppose. In Chapter III., which deals with Perceptional Intuitionism and Subjective Morality, he recognizes that moral perceptions are only the raw material of ethical theory, and that some moral perceptions are not valid. The problem then is to determine what moral perceptions are and what are not valid. This question is partly answered by the distinction between subjective and objective morality, which Mr. Mezes very properly emphasizes. Actions are genuinely moral (*i. e.*, subjectively) when conscience approves them (53). But since some moral perceptions are not valid, action in accordance with conscience is not always fully and adequately moral (*i. e.*, objectively moral) (54). Every action that is moral at all is subjectively moral, while objectively moral actions not only follow their agent's conscience, but also the approval of the wise conscience, to which in such cases the agent's conscience conforms (55). It thus appears that the standard of objective morality is not what men hold to be right, but the conscience of the wise man. But who is the wise man? He is one who 'will in his actions serve the supreme end, and will in his insight view it,' though even he cannot describe it fully (382). Since, then, ethics refers us ultimately to the wise man for our knowledge of what is fully and adequately moral, and since the wise man is one who knows and follows the supreme end, which is given a fairly definite content by the inclusion of the cardinal virtues, I do not see why we

may not refer directly to this supreme end as the norm, and judge conduct by its conformity or non-conformity to this standard.

Fundamental to Professor Mezes' system is the distinction between subjective and objective morality, already noted; and since he holds that it is on a basis of subjective morality that the more difficult task of understanding objective morality can best be approached (55), he first treats of the former. The book is accordingly divided into two Parts: Part I. dealing with subjective, and Part II. with objective morality.

Voluntary action and conscience, which are the two parts of subjective morality (55), are the subjects dealt with in Part I. Only one chapter is devoted to voluntary action, because this subject is well treated in works on psychology. The chapter is slight and does not call for comment. Mr. Mezes apparently follows Professor James in his theory of the emotions and in his view of the rôle of effort in the moral life.

The discussion of Conscience, which occupies the remainder of Part I. and about a fourth of the book, begins with a chapter on the the Adult Conscience. Professor Mezes accepts the division of conscience into an emotional and an intellectual aspect, and then proceeds to describe the principal component emotions and ideas. The most important moral emotions are responsibility, obligation, free-performance, and feelings of approval and disapproval, the three former appearing only when the agent's actions are in question, the two latter generally appearing when the actions of others are regarded. The intellectual elements of conscience are ideas that awaken the emotions just named, *i. e.*, such ideas, namely, as justice, temperance, veracity, etc. Conscience, however, is not to be identified with obligation, remorse, or any of its components, but rather with the moral ideal. "The moral ideal is the dynamic, schematic system of such ideas of action as awaken approval or disapproval, responsibility, obligation, and the sense of free-performance" (90). In other words, we understand Professor Mezes to mean that conscience involves the consciousness of a practical ideal, together with a felt obligation on our part and that of others to live up to it so far as in us lies, successful effort in this direction calling forth moral approval, while failure or lack of effort is followed by disapproval. Professor Mezes' analysis apparently takes cognisance of all the essential elements of conscience, and is sane and sound; but it is surely unnecessary, and moreover unlike the author's usual directness, to resort to such a style of exposition as is illustrated in the definition just quoted, or in the following: "An ideal is an epit-

omized biography of an exemplar, or a composite biography of many exemplars acting in some particular capacity."

In Chapter II. it was shown that only voluntary actions are moral phenomena. But while no phenomena except voluntary actions awaken conscience, some of these fail to do so. The question accordingly arises: "What voluntary actions are and what are not morally judged?" This question is answered in Chapter VI., which maintains that conduct is the cause of conscience. Actions done from caprice, preference and prudence, though voluntary, do not arouse conscience. This is because the effects of such actions vitally and evidently affect only the agent and not others. Conscience appears only when the agent becomes aware that his action vitally affects the interests of others. Accordingly conduct, which is said to be the psychic cause of conscience, is defined as 'apprehended voluntary action that the apprehending subject views as affecting others in interests he considers vital' (98). Now Professor Mezes undoubtedly has the right to define the terms he uses; but aside from the fact that this seems an unusual limitation of the import of the word conduct, the position just stated seems alike contrary to fact and to the author's fully developed view. For, as he immediately remarks, 'it is generally recognized that each man owes duties to himself,' and that 'self-respect demands such and such conduct.' "Conscience is undoubtedly aroused when a contemplated action threatens to take from the agent's moral power and quality." Injury to the self of prudence does not arouse conscience, but injury to the moral self does arouse it (98, 101). The apparent contradiction between these two views is explained by saying that 'a man objectifies his moral self and looks upon it as an *alter ego*, and moreover that his moral self is really the representative of the interests of others, and, therefore, that in protecting the moral self conscience protects an *alter* and indirectly protects the interests of fellow men' (101). With the greater part of this statement we are in agreement. In no sphere has modern psychology labored more successfully than in showing the social origin and nature of the individual self. Nevertheless it is important to distinguish between the real relation between the individual self and the *alter ego*, and the conscious recognition on the part of the individual of this relationship. A relationship which psychology or metaphysics with much patient study discovers, is not necessarily present even to the reflective consciousness, and the statement that in order to awaken conscience a man must objectify his moral self and look upon it as an *alter ego* which represents the interests of others, is a very

questionable psychological proposition. Without resorting to a philosophical explanation of the relation of the individual to the social self in order to reconcile two apparently conflicting views, it is surely truer to fact as well as 'a simpler course' to adopt Mr. Mezes' alternative definition of conduct 'as action that is apprehended by the agent as vitally affecting other persons, quasi-persons, or his own moral self.' Even this definition of conduct is, I think, too narrow; but it has at least the advantage of making it easier to recognize character as an object of moral judgment. Voluntary actions are 'the indication and embodiment of character'; but if character is itself a moral phenomenon of supreme importance, the character of the agent is an important moral phenomenon, and is regarded by himself as, in Mr. Mezes' phraseology, a cause of conscience. It is only in virtue of the 'simpler course' of interpretation above mentioned that Professor Mezes can be acquitted of confusing the question of the psychic cause of the adult conscience, *i. e.*, 'what mental states have the dynamic efficiency necessary to arouse it' (91), with the question of the origin of conscience.

The ontogenetic question is treated in Chapter VII. on the 'Birth and Growth of Conscience in the Child.' Since conscience is made up of ideas of moral agency, the author is led to ask, first, How does the child come to get ideas of agents? and secondly, How does he get ideas of moral agents? (107, 118). Throughout the chapter the author pretty closely follows Professor Baldwin's exposition through the 'projective,' the 'subjective' and the 'ejective' stages, teaching that the child gradually learns to be an aggressive agent, a submissive agent, a social agent, and finally, through obedience and imitation, a moral agent.

The purpose of Chapter VIII. 'is to discover the conditions under which conscience appears and develops in the race.' There is general agreement that conscience is present in all men, and absent in all animals. "It may be, then, that conscience is one of the differences, or that it grows out of one or more of the differences between men and other animals." At all events, in attempting to distinguish between moral and non-moral phenomena, we are confronted by the fact that the actions of men are commonly regarded as moral phenomena, while the actions of brutes are not so regarded; hence we are led to inquire what are the differences between men and other animals in virtue of which the former are pronounced to be moral agents and the latter are not. The mental differences which Mr. Mezes gives are speech, judgment, self-consciousness and effort. But since self-

consciousness is the most important of these and implies an advanced stage of the mental life with which the other proper differences are inseparably bound up, or of which they are only different aspects or expressions, I should prefer to treat the other differences in relation to self-consciousness as the focal point. Mr. Mezes indeed makes self-consciousness the fundamental difference, since he regards it as the condition of speech and judgment. It is admitted, he says, that animals use language, but man alone has the power of speech. What, then, is the difference between language and speech? The power of making judgments is the essence of speech, and self-consciousness is the essence of judgment (141). Mr. Mezes also says (142) that speech is made up of assertions, and that assertion is a self-conscious procedure. Hence animals not possessing self-consciousness are incapable of speech.

But when it is said that speech is 'an *attempt* or *endeavor* to convey by signs some idea of certain facts,' and that animals, though they may convey meaning, do not *intend* to do so (142), these statements are not only questionable, but they do not represent 'only the points of agreement' among authorities to which Professor Mezes claims to restrict himself in this connection. "Consider, for instance, the mere groans of pain of the solitary hunter accidentally shot in the forest, and compare with them the groans he gives later to tell an unskilled nurse that his position is uncomfortable. The first groans are significant enough, and would be understood by any one at hand, even the hunter's dog; they are in fact like the language cries of animals. The second groans assert discomfort, and are human speech" (141f.). The word 'assert' implies self-consciousness; but the latter term is not yet defined, and in the light of the illustration and definition of speech just given comparative psychologists would not agree in denying to animals the use of speech. A dog that is ill will sometimes lie quite quietly when no one is in the room, and then when its master comes in will utter groans apparently for the purpose of indicating that it is suffering and of evoking some expression of sympathy. When a dog with an injured foot simply goes about the house on three legs, this fact conveys a meaning; *we* obtain from the fact an idea which the dog does not intend to convey. But when the same or another dog comes directly to its master and holds up its wounded paw, I cannot doubt that this is an attempt to convey by a sign an idea of the fact that its foot is injured, and that the dog *intends* to convey this meaning. Or, take the illustration quoted in this connection (p. 139) from Romanes: "Terrier A being asleep in my house,

and terrier B lying on a wall outside, a strange dog, C, ran along below the wall on the public road following a dog-cart. Immediately on seeing C, B jumped off the wall, ran upstairs to where A was asleep, woke him up by poking him with his nose in a determined and suggestive manner, which A at once understood as a sign; he jumped over the wall, and pursued the dog C, although C was by that time far out of sight, around a bend in the road." Now if in this instance the dog B suggested to A only what we symbolize by the word 'Come,' as Lloyd Morgan holds, we have at least an illustration of what Morgan calls 'indicative intercommunication,' and what Romanes calls 'intentional sign-making,' which is common to animals and human beings.

It is not necessary to multiply illustrations; it is more important to observe that according to the view of the development of mental life expounded in Chapter VII., as well as in accordance with the opinion of competent observers generally, the child not only employs intentional sign language to express its wants and emotional states, but also begins to talk and uses phrases '*in order to communicate* information * * * to other minds' (Romanes) for some time prior to the rise of the consciousness of self. An animal or a child, in short, may *intentionally* convey meaning, may endeavor to convey some idea of certain facts; but this meaning is only the expression of the self of the moment; the child or the animal does not consciously refer its acts to itself as the permanent subject of them, nor can it reflect upon and analyze the conscious states which it may have had at the time it conveyed the idea of certain facts. Professor Mezes would agree that this reflective self-consciousness separates man from other animals, but as I understand him he would draw the dividing line lower down, at volition. And in summing up what has been said of the mental differences between man and brute, we are told that 'effort, the essence of volition, differences men from animals' (149). Here apparently we have the difference reduced to its lowest terms. We have already been told (65) that the feeling of effort is a sensation which is made up in part of sensations of movement and in part of cenesthetic sensations; but Mr. Mezes, of course, would not deny that animals have these sensations, though I think it would add to the clearness of his exposition if he expressly distinguished between muscular and volitional effort. When he says (145) that 'man can put forth effort, can try, can set his teeth and square his jaw to accomplish what he wills, while animals have no such power * * * ' we must lay the emphasis upon 'wills' and remember that 'to mean something and to intend or will it is the same' and that animals do not intend or mean anything.

This is not the place, even were I competent, to criticise Mr. Mezes's view of the rôle played by effort in human and animal life; but it may be noted that as most psychologists would agree with Romanes that animals intentionally convey meaning, so I believe they would agree with Lloyd Morgan that the dog which might have bitten him while he was sewing up its gashed side, but did not do so, possessed a kind of volition (perceptual) involving conscious effort, though not the capacity for 'introspective reflection in the light of self-consciousness.'

One other point may be mentioned in this connection. When Mr. Mezes says that 'animals possess general ideas,' it would be well to define what he means by general ideas. For if by general ideas he means 'abstract' ideas, Lewes and Mivart deny that animals possess them, while Wundt thinks the evidence is on the whole against it. Lloyd Morgan and Romanes draw the dividing line between man and brute in regard to the possession of abstract ideas at different places, the former denying, and the latter holding, that animals are capable of forming abstract ideas in the sense of what Principal Morgan calls 'conceptual isolates.' And even Romanes denies that animals possess general ideas in the sense of 'concepts,' since these are conditioned by the presence of that kind of self-consciousness which animals lack.

Part II., which deals with the *summum bonum*, must be more briefly treated. Men have always agreed that there is some one thing which is of supreme worth, and theories of this one supreme thing fall roughly into two classes, viz., eudæmonistic theories and perfectionistic theories (p. 190). Following Aristotle, Professor Mezes quite correctly identifies eudæmonism with welfare, and not with happiness in the purely hedonistic sense. But when he says that Aristotle's account has been long and much neglected, and that 'it is difficult to point out any one since Aristotle who has not been guilty of the confusion' between happiness and welfare (p. 398), one must demur. Moreover, since Professor Mezes expressly distinguishes eudæmonism from hedonism, what room is made for hedonism in the above rough classification? When he speaks of 'the difference between eudæmonists, perfectionists and believers in a complex end' (p. 193), he would seem to identify welfare with happiness and to overlook the fact that *εὐδαιμονία* is itself a complex end. Instead of saying 'the good is complex, being a combination of welfare and perfection,' I should prefer to say that perfection or virtue is the most important constituent of the complex end, welfare. Mr. Mezes describes his own theory of the good as 'sentient welfare'—by which he means the welfare of all sentient

beings. But I think the phrase unfortunate because ambiguous, since it might be taken to mean the welfare or good of the sentient nature of man, and would thus prove misleading.

Having determined that sentient welfare is the ultimate end, this, of course, becomes the leading criterion of objective morality. But there are also other criteria. There are a number of sources from which accounts of the trustworthy components of conscience, *i. e.*, such demands of conscience as make for objective morality, could be derived. But the doctrine of the cardinal virtues furnishes the most reliable and convenient account. 'A virtue is a quality of character that insures the performance of objectively moral conduct,' and since 'the doctrine of the cardinal virtue undertakes to give a full account of the perfectly moral character,' this furnishes the best foundation for a theory of objective morality (201, 202). The greater portion of Part II. is accordingly occupied with a discussion of the four cardinal virtues, to which the author adds benevolence. Mr. Mezes seems to me at his best in the kind of discussion for which these chapters afford opportunity. It is, of course, impossible to review them at length, but one or two points may be noticed by way of criticism.

In the chapter on Courage, Mr. Mezes says, 'the habit of moral courage is subjectively right and in so far approved, but is also at times injurious rather than helpful, and cannot, therefore, be called a virtue' (220). But if this be so, I do not see how veracity or even benevolence or justice can be called a virtue. Justice, benevolence, and veracity are qualities of character which are undoubtedly 'helpful' on the whole and tend to promote objectively moral conduct. But any of them may *at times* prove injurious to the larger interests of society, so far at least as our limited view can judge. It may, indeed, be contended that the good or evil consequences of an action —*i. e.*, its objective morality—are exactly proportionate to the good or evil in the motive, as T. H. Green would teach. But this view proceeds upon the supposition that moral conduct is measured in terms of perfection of character. Measured in any other way, good motives may always at times lead to injurious consequences, and to deny that moral courage is a virtue because it is at times injurious rather than helpful in its effects, seems to me to confuse two things which Professor Mezes is elsewhere careful to distinguish, viz., 'a quality of character with the conduct to which it leads' (p. 219).

The object of the chapter on Temperance is to discover and formulate the rules which should govern the appetites and desires, in

accordance with the demands of objective morality. The three fundamental and typical appetites of food, drink and sex, which are distinguished from the 'lesser desires' (p. 222), form the subject-matter of this chapter. An appetite is defined as 'a desire with a massive bodily basis,' and in the discussion the terms appetite and desire seem to be employed as synonymous. The failure to distinguish between appetite and desire accounts for the omission of what seems to me an important factor in explaining the use of intoxicants, viz., the influence of organic craving, which I should distinguish from the 'desire for heightened consciousness.'

The longest and most important chapters in Part II. are those on Benevolence and Justice. The ordinary meaning of the former word is extended, while that of the latter is usually, though not with thorough consistency, restricted to legal justice; with the result that some topics which would ordinarily be included under justice are treated under the head of benevolence. In Mr. Mezes' terminology benevolence 'includes all inclinations to objectively moral conduct.' But since it is easier to discover what deeds are allowed than it is to know what inclinations (or feelings) are approved, if we first discover what conduct is sanctioned, we may then infer what opinions are entertained towards the feelings that prompt it (265–8). Law thus becomes the criterion of objective benevolence; and since justice is the moral quality with which the law preeminently deals, it would seem to me more natural to discuss Hostility to External and Internal Public Enemies under the category of justice. Almost the entire subject-matter of the chapter on Benevolence is ordinarily dealt with in works on International and Private Law. When Mr. Mezes remarks that international law recognizes the principle that nations must have their 'quarrels just,' and proceeds to state the conditions precedent to a 'just war,' he is using the word 'just' in the moral and not the legal sense to which he by definition restricts it; but he is also recognizing the principle that justice and not benevolence is the basal concept in international law. Indeed he habitually speaks of benevolence where ordinary usage would suggest the word justice, as in the statement that 'benevolence has always required gratitude for benefits received' (296), and that the benevolence of gratitude rests on a basis of desert (299).

Professor Mezes defines justice as 'that portion of morality that consists of customs and usages that can be embodied in a code of laws,' and the just man as one 'who knows and lives up to this code.' Yet he does not identify moral and legal justice, as these

statements might seem to imply. He recognizes that systems of justice—*i. e.*, legal systems—do not embody ideal justice (303), and that the law and finding of the courts is not always just (314); but since aside from the courts there nowhere exists a sustained and coöperative effort to work out the idea of justice in detail, he holds that legal justice furnishes the best basis on which to found moral justice (209). I should say that the relation is precisely the other way. But when it is clearly recognized that legal justice is not to be taken as the measure of moral justice, that the latter frequently demands a modification of the existing code, and that there are 'moral actions the courts cannot consider and enforce,' no objection need be taken to Mr. Mezes's method for the purely descriptive purpose he has in view. Justice as embodied in codes of law furnishes a good basis for the description of kinds of conduct commonly regarded as just, though I do not see how justice so conceived, and with the limitations above indicated, 'may serve as an authoritative moral guide to all peoples and nations' (326). In his effort to make it serve this purpose, Mr. Mezes is in some danger of lowering the standard of moral justice, as, for example, when he says that the general legal and *moral* rule is *caveat emptor* (353). Nevertheless, the chapter on Justice, of which the author speaks with diffidence in the preface, is perhaps the most original, painstaking and thorough chapter in a book which covers a wide area and deserves careful study. It is no light work to have accomplished the task Mr. Mezes had set himself to do, with the success which he has achieved.

George S. Patton.

Princeton University.

NEW BOOKS.

The Varieties of Religious Experience. A Study in Human Nature, being the Gifford Lectures on Natural Religion delivered at Edinburgh in 1901–1902. WILLIAM JAMES. New York, Longmans, Green & Co. 1902. Pp. xii + 534.

Studies from the Yale Psychological Laboratory, edited by E. W. SCRIPTURE. Vol. IX. Researches on the Rhythm of Speech. New Haven, Yale University. Pp. 143.

University of Iowa Studies in Psychology, edited by GEORGE T. W. PATRICK. Iowa City, The University. 1902. Volume III. Pp. 144.

Die Analyse der Empfindungen und das Verhältniss des Physischen zum Psychischen. E. MACH. 3d Enlarged Edition. Jena, Fischer. 1902. Pp. ix + 286.

Ueber Annahmen. A. MEINONG. Leipzig, Barth. 1902. Pp. xv + 298.

Studien zur Werttheorie. ROBERT EISLER. Leipzig, Duncker & Humblot. 1902. Pp. xii + 112.

Principles of Western Civilization. BENJAMIN KIDD. New York and London, The Macmillan Company. 1902. Pp. vi + 538.

NOTES.

THE University of Edinburgh has conferred the degree of LL.D. on Professor William James and on President J. G. Schurman.

PRESIDENT NICHOLAS MURRAY BUTLER has received the degree of LL.D. from Johns Hopkins, Princeton, Pennsylvania and Yale Universities.

PRESIDENT F. L. PATTON has resigned the presidency of Princeton University, but retains the chair of ethics.

MISS MARGARET F. WASHBURN, Ph.D. (Cornell), has been elected to the professorship of psychology in the University of Cincinnati vacant by the removal of Professor Judd to Yale University. Miss Washburn is at present warden of the women's college at Cornell University and will occupy a similar position at Cincinnati.

Howard C. Warren has been promoted to a full professorship of experimental psychology at Princeton University.

At Cornell University, Dr. Ernest Albee and Dr. Albert Lefevre have been promoted to assistant professorships in philosophy, and Dr. I. M. Bentley to an assistant professorship in psychology.

A psychological laboratory has been established at Mt. Holyoke College, five rooms on the top floor of Williston Hall having been assigned for this purpose. A substantial equipment of apparatus has been provided and enlargements are being rapidly made. The work is under the immediate charge of Dr. Thompson. Dr. Talbot is at the head of the department.

A branch of the Psychological Association was established at Chicago on April 19, 1902. A committee consisting of Professor Coe, of Northwestern University, Professor Dexter, of the University of Illinois, and Professor Angell (chairman), of the University of Chicago, was appointed to arrange for a meeting during the coming year.

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THE PSYCHOLOGICAL REVIEW.

STUDIES FROM THE PSYCHOLOGICAL LABORATORY OF THE UNIVERSITY OF CALIFORNIA.

COMMUNICATED BY PROFESSOR GEORGE M. STRATTON.

III. VISIBLE MOTION AND THE SPACE THRESHOLD.

BY PROFESSOR GEORGE M. STRATTON.

For some time it has been argued that the perception of motion has no immediate connection with the discrimination of positions in space. It is held that the two processes are psychologically independent, and that we become aware of motion by a direct and simple sense of the motion itself, and not by appreciating that the object occupies distinct localities.

This paradox is supported in part by the fact that a movement in the out-lying portions of the visual field can be readily seen when the movement has a far less extent than is required to give to two motionless objects a perceptible difference of position,—a fact apparently first observed by Exner. To state the matter in the words of Professor James: "One's fingers when cast upon the peripheral portions of the retina cannot be counted—that is to say, the five retinal tracts which they occupy are not distinctly apprehended by the mind as five separate positions in space—and yet the slightest *movement* of the fingers is most vividly perceived as movement and nothing else. It is thus certain that our sense of movement, being so much more delicate than our sense of position, cannot possibly be derived from it. * * * Movement is a primitive form of sensibility."[1]

[1] 'Principles of Psychology,' II., 172.

One may rightly have some antecedent hesitancy, I believe, in regard to James' interpretation. Even admitting the facts he offers, one need by no means draw his conclusion. For the alleged sense of motion, if it really is a sense of *motion*, brings in a spatial report. The changes of which it makes us aware are preceptibly different from alterations merely of intensity or of color. Even though we may be unable to tell the direction of the motion, the motion itself is a change of position, and is dimly appreciated as such. Instead of saying, then, that the experiments cited are evidence that the sense of movement is so much more delicate than the sense of position, it would be more exact to say that they show that the discrimination of positions during movement is much finer than the discrimination of positions at rest. It is not really an antithesis between 'motion' and 'differences of position,' but between differences of position under two contrasting sets of conditions, motion being but a special mode of testing our power of local discrimination. The truth seems to be that there are various ways of measuring this power—among others, by the simultaneous presentation of two lights in different places, by their successive presentation, or by a continuous movement of a single light from one position to another. We have no right to assume (as Professor James seems to do) that the first of these methods is the only one that gives the true space-threshold, and that the results of the third, if finer, are indications of a process different in kind. The second method also gives finer results than the first, and yet no one, so far as I know, has thought that the finer space results obtained by successive stimulation implied some special and primitive form of sensibility different from that which is involved in discriminating simultaneous impressions. Why, then, should we jump to this conclusion when the conditions of space-perception are only slightly altered farther, making the successive stimulation spatially continuous instead of discrete?

Admitting the facts adduced, then it by no means follows that we have a primitive sense of movement, independent of spatial discrimination. But at least so far as space is concerned, the facts themselves are not unquestionable. Some experiments

by Stern, reported in 1894,[1] already raised a doubt here. Stern found that when retinal irradiation was decidedly reduced, the shortest perceptible movement was not appreciably less than the space-threshold as determined without movement. He consequently inferred that there was no ground for assuming a specific and unique sense of movement. But the test of motionless space-discrimination used by Stern is itself perhaps not fully convincing. He departed from the method employed by Helmholtz and others — the method by parallel lines brought closer and closer together until they almost fused — and, instead, took as the threshold that width of a single dark line that was just doubtfully perceptible against a light background. The assumption here seems to be that the line is a gap or interruption of the white surface, and implies a local discrimination of the two borders of white against the line which divides them. But if we regard black as a positive impression, as it would seem we must, there would appear to be no reason why a consistent development of this method would not require us to accept the apparent diameter of a just perceptible fixed star as a still more accurate measure of local discrimination. The width of the dark line in Stern's experiments is probably of importance only in a secondary way, by affecting the intensity of the impression of black. This suspicion is strengthened by the fact that with some slight improvements in the conditions of observation in our laboratory, while still remaining true to the general principle of Stern's method, the threshold takes an astonishing drop. One of my students, Mr. Gilbertson, finds that the width of the black line visible against white, instead of being 15″ angular measure, as Stern found under his conditions, may be even less than 2.5″.

For this reason it seemed well to try some experiments in which the threshold of motion and that of local discrimination might be compared without so much doubt as to the really spatial character of this local discrimination. The experiments here reported fall into two groups, the first with indirect, the second with foveal vision.

[1] Die Wahrnehmung von Bewegungen vermittelst des Auges, *Zeitsch. f. Psychol. u. Physiol. d. Sinnesorg.*, VII., 321.

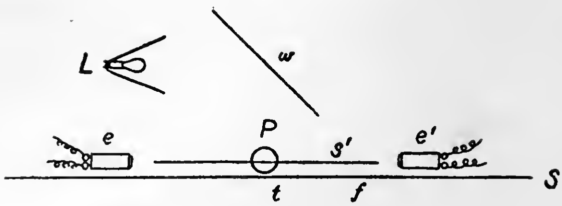

FIG. 1.

I. *Experiments with Indirect Vision.*—The observer, in Fig. 1, sat in a half-darkened room and judged the character of the light-stimulus seen through a narrow vertical slit *t* about ½ mm. wide in a screen *S* before him. The fixation point *f* was 5° to the right of the slit in one set of experiments and 30° to the right in the other, the distance from the observer to the screen being in these two cases 2 m. and 1 m., respectively. When the aim was to determine the threshold of motion, a bright point of light moved from a fixed position upward or downward in the slit, while the local discrimination was tested by two separate motionless points of light which appeared the one above the other and in immediate succession. The extent of movement, in the one case, and of spatial separation in the other, were of course accurately varied to determine the threshold.

This variation, along with a constancy of those conditions that should remain constant, was brought about by an arrangement behind the screen, consisting chiefly of a pendulum *P* carrying a smaller slitted screen *S′*, which in swinging past the slit *t* allowed light to pass to the observer from a white surface *w* evenly illuminated by an electric lamp, *L*. The pendulum was so controlled by the electromagnets *e* and *e′* as to give a single swing from a fixed point 10° from the vertical and be caught at the end of its course on the other side. The back-swing in preparation for the next experiment was concealed from the observer. The length of the pendulum was regulated to give a single oscillation in one second.

In Fig. 2 there is represented in diagram the screen *S′* that was carried by the pendulum when the aim was to give the observer a moving point of light. The main feature of this

screen was a movable circular disc *D*, from whose center, *c*, there passed to the periphery a slit *cp*, ½ mm. in width, forming an arc of the circle whose radius was the distance from *C* to the point of suspension of the pendulum (70 cm.). When the

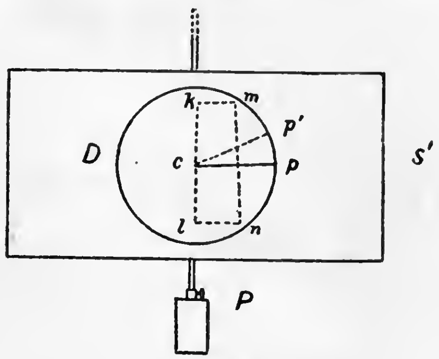

FIG. 2.

disc was so set that the center of this arc coincided with the point of suspension of the pendulum, the point of light which came to the observer as this arc-slit swung past the vertical slit *t* in the stationary screen *S* in Fig. 1 had no apparent motion whatever. A change of the disc's position, however, so that the slit took the position *cp'*, would cause an apparent upward motion of the point of light, the extent of which in angular measure, from the point of view of the observer, could readily be calculated. A scale in terms of such angular measurement was placed along the circumference of the disc so that this could be set for any extent of motion desired. But since it seemed best to keep the duration of the movement exactly the same as the duration of the stimulus in the corresponding set of experiments with motionless points, this constancy, in spite of the varying positions of the disc, was maintained by the fact that the screen *S'* was continuous behind the disc *D* with the exception of an opening *klmn*, whose boundaries *kl* and *mn* were radii of the circle mentioned above, the center of which was the point of suspension of the pendulum. It need only be added that this screen was wide enough to conceal the surface *w* in Fig. 1, when the pendulum was at either limit of its excursion.

For the experiments on the discrimination of position without movement, a different screen was substituted upon the pendulum, yet with a similar arc-slit similarly centered. But in this case, instead of revolving the slit about a central point, one half of the slit could be shifted slightly up or down, along the radius of the circle of which the arc was a portion, as in

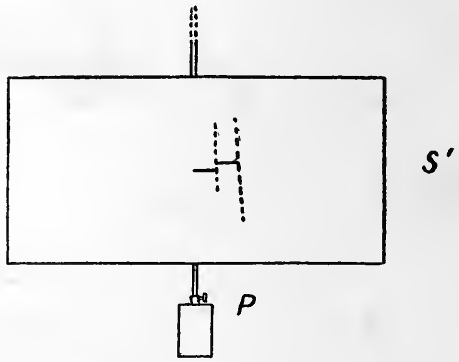

FIG. 3.

Fig. 3. As the pendulum made its single swing, there thus appeared to the observer in immediate succession two separate points of light, the amount of whose separation could be accurately varied by the experimenter.

In the preliminary experiments, the well-known method of 'minimal changes' was employed, but the observers soon showed themselves so much influenced by expectation that no reliable determination of the thresholds seemed possible by this means. The method of 'serial groups,' hereinafter described, was found more satisfactory. The adoption of successive stimuli to measure the local discrimination without movement, was for the purpose of avoiding some of the worst difficulties from irradiation. Two points of light when presented simultaneously will often produce a blur that is perceptibly extended (and consequently spatial), and yet two separate nuclei of light are not distinguished within this vague extent. According to the usual method of interpreting the results, we should here be below the space-threshold, although, of course, we are not. What Tawney has already justly insisted on with regard to

tactual impressions,[1] that the perception of two points is not a true measure of the threshold, might consequently be repeated with respect to vision. The fusion of the two points is largely obviated by the use of successive stimuli so that the intensity of the first impression is perceptibly lessened by the time the second is at its height.

But the substitution of successive for simultaneous impressions brings what some might consider a vitiation of the results. The two points of light, even when well separated, give the psychological impression of continuous motion; the light seems to leap from the position of the first point to that of the second. At the beginning it was thought possible to separate those judgments in which an apparent motion entered, and treat them as a questionable group by themselves. But when once the suggestion of motion becomes fairly lodged in the observer's mind, it occurs so persistently and over such a range that this attempt was renounced. Since there is no objective movement here, however, and the whole thing is really a matter of suggestion pure and simple, and seems to me to imply an underlying space-discrimination as the basis of the suggestion itself (as I have urged at the beginning of this paper), the judgments with subjective motion have been freely employed in computing the thresholds. Those to whom these reasons do not seem sufficient may have no such scruples over the second group of experiments on foveal vision; not even suggested movement there enters into the judgment.

The results of the experiments with indirect vision were as follows:

Observer.	Angle of Observation.	Number of Determinations.	Discrim. of Position.		Perception of Motion.	
			Threshold.	M. V.	Threshold.	M. V.
A	5°	6	9.2′	1.6′	10.4′	1.4′
	30°	5	29.0′	5.2′	29.0′	3.2′
Bd	5°	6	7.9′	.7′	8.3′	1.9′
	30°	3	18.3′	2.2′	21.7′	5.6′
Bl	5°	3	7.5′	3.3′	6.6′	2.2′
D	30°	4	63.7′	8.7′	70.0′	10.0′

[1] 'The Perception of Two Points Not the Space Threshold,' PSYCHOL. REVIEW, II., 585.

The table indicates that where the conditions are practically equal, the perception of movement has no advantage over the discrimination of position without objective movement. So far as indirect vision is concerned, the theory that the two processes are psychologically independent here finds no support.

II. *Experiments with Direct Vision.*—The second group of experiments dealt with the same problem as the preceding, with a change merely in the method of investigation and with a different portion of the retina—the foveal instead of the excentric region of sight. On account of the extraordinary nicety of our space discrimination under these conditions, the observer was placed in a distant building where there was an unobstructed view of one of the laboratory windows, the distance of his station from the object to be observed amounting by accurate survey to 120 m. The observer faced the north, and the portion of the laboratory towards which he looked sent back no direct glare from the sun; preliminary experiments showed the need of regarding these things. The distance of the observer's station, it is true, was inconvenient in many ways, but this was more than offset by the greater ease of observation. The naked eye could now be employed, and all the difficulty was avoided that comes from the use of a reducing lens, such as Stern found necessary. The fact that the threshold of movement easily ran down to about one half of what he reports shows the advantage of these conditions. For the perception of motion a strip of white bristol-board, 8 mm. wide by 50 cm. long, was mounted vertically on a dead-black frame which could be moved horizontally before a larger dead-black background. From the observer's station the frame itself was invisible, aud all that one saw was the white line on the large black field. The movement of the line was controlled by runners and guides and adjustable stops on the frame, so that the motion was kept horizontal and its excursion varied by steps of 1 mm. The experimenter moved the frame by hand, and during any single observation kept the extent of the movement constant, and continued the oscillations from the moment just before the exposure of the line until the observer had signalled his judgment. In order to insure full justice to this side of the investigation, the *rate* of movement

had, also, of course, to be taken into account; otherwise some velocity might be selected that would not be the most favorable to perception. For this reason the experiments on motion were subdivided into five groups, with a rhythm, respectively, of 60, 100, 180, 300, and 450 single swings to the minute. The rates were maintained with reasonable accuracy by the aid of an adjustable pendulum invisible to the subject of the experiment.

For the corresponding set of experiments on the discrimination of position without movement the conditions were in all respects the same, except that for the moving frame with its single vertical line two vertical lines end-to-end against the dark background were substituted. Of these two lines the lower was fixed, while the upper was capable of being shifted from continuity with the lower line to any position to the right or left, remaining throughout parallel, however, to its original position. The actual settings varied by steps of 1 mm., and the observer had simply to judge whether the two lines at any given setting were continuous, or in what respect they spatially differed.[1]

The results of the two sets of experiments under these conditions were as follows:

Discrimination of Position Without Movement.
Thresholds in mm.

Observer S.		Observer Y.	
Left.	Right.	Left.	Right.
4	4	6	3
7	5	4	6
3	5	5	6
7	5	6	6
3	3	7	4
4	6		
Av. 4.3	4.3	5.6	5.0

The thresholds for the discrimination of position without movement are slightly higher than those obtained in my previous study already referred to, but the difference is not such as to call for any special comment. The thresholds for the perception of movement, as affected by the rate of move-

[1] For a more detailed account of this method, see my 'New Determination of the Minimum Visibile and its Bearing on Localization and Binocular Depth,' Psychol. Review, VII., 429.

PERCEPTION OF MOVEMENT.
Thresholds in mm.

Single Oscillations per Minute.	60	100	180	300	450
Observer S.	11	6	4	4	4
	16	4	4	4	5
	14	7	4	4	4
	8	5	4	4	6
Av.	12	5.5	4	4	4.75
Observer Y.	25	14	7	4	7
	15	14	5	6	7
	18	8	6	6	6
	16	12	6	5	6
Av.	18.5	12	6	5.2	6.5

ment, take a somewhat different course from those reported by Stern.[1] This investigator worked with rhythms of 144, 84 and 72 vibrations to the second, and found *merkwürdigerweise* (as he says) that the slowest movement was the one most distinctly perceived. In my own results, it will be seen that the lower rates, those of 60 and 100, are markedly unfavorable to perception, with a decided improvement as we pass to 180 and 300, the threshold again rising slightly as the rate is still farther increased. Virtue here seems again to lie in the mean. The lower rates require a considerable excursion before they can be distinguished from rest, while the most rapid oscillations tend to produce a mere indistinctness rather than a perceptible swaying of the object. I am quite at a loss to account for our divergent results here, unless it be that by the word '*Schwingungen*,' which Professor Stern uses without modification, he means '*double* vibrations.' In this event his lowest rate would be about midway between my 100 and 180, and he would have caught only the dip of the threshold as it comes out of the region where the rapidity of the impressions produces blur, without his rates becoming slow enough to show the upward trend of the threshold again farther on. My own subjects, like his, never knew beforehand the rate that was being used, so the discrepancy cannot be explained in this way. Possibly the difference is in some way connected with the fact that his observers had to look through the lens of a microscope.

[1] *Zeitschrift für Psychologie u. Physiol. d. Sinnesorg.*, VII., 347.

For observer *S*, the most favorable rates of motion give a threshold of 4 mm. (about 6.8″ angular measure) as against 4.3 mm. (about 7.3″) for the discrimination of place — a net advantage, for motion, of .3 mm. or .5″ of arc, on the average. If, however, we were to disregard averages and look at the individual determinations, it would appear that the space discrimination occurred as low as 3 mm. (5.1″ of arc); while in no case does motion become perceptible below 4 mm. For observer *Y*, the mean threshold for position without movement is 5.3 mm. (9″); while the most favorable rate of motion gives 5.2 mm. (8.8″) on the average. Here again the minimum thresholds for position are below the smallest for movement. Thus, from one point of view, motion has a slight advantage over discrimination without movement, which advantage, however, is reversed when the numbers are differently regarded. But in either case the difference is too slight to serve as a sufficient basis for entirely differentiating the psychological processes involved.

The results here, then, are in substantial agreement with those obtained in indirect vision by an independent method. The doctrine that visual motion is a primitive form of sensibility independent of local discrimination finds no experimental warrant. The perception of motion seems to be nothing more nor less than the perception that a sensation is changing its space relations, the motion itself furnishing a decidedly favorable, but by no means unique, set of conditions for appreciating such differences of space relationship. This does not imply that the detection of movement always involves a deliberate comparison of positions; for the discrimination often undoubtedly occurs at a single psychic stroke. But even this apparently simple stroke is really a complex act. It implies a relational activity of the mind which interprets and gives character (crude and confused though it be) to the incoming sensations, so that they are no longer blank impressions, but are impressions which mean for us *movement*. The experiments thus go to support the view that a fact of space can never be conveyed to the mind in the form of a pure sensation divested of all relationship.

IV. The Method of Serial Groups.

BY PROFESSOR GEORGE M. STRATTON.

In the practical conduct of the laboratory one frequently feels the shortcomings of the method of 'minimal changes.' It is undoubtedly the best all-around mode of procedure yet devised, but in certain cases where the conditions are exceptional it may leave one quite *im Stich*. This is especially true when one is dealing with minimal impressions, where suggestion is apt to find such free play; the observer may continue to notice a sensation when the stimulus has become suspiciously weak—in fact when no stimulus at all is applied. Thus, with certain excellent though suggestible subjects, I have found it impossible to determine by the method of minimal changes, pure and simple, the least extent of visible motion that could be perceived as motion. The subjects persisted in seeing the light move on every occasion, whether there was any actual movement or not. The control of the answers, by requiring that the observer shall tell correctly some additional feature of the impression—tell, say, the direction of the movement, or, if the experiments be on the least perceptible change of pressure, tell whether the pressure becomes heavier or lighter — may in some instances be helpful. But often this check will hide the very facts that one wishes to ascertain — the point at which the subject perceives motion and yet is uncertain of the direction, or notice change of pressure without being able to say whether the weight has grown greater or less.

The usual resort in this event is either to a so-called 'catch experiment' the *Vexirversuch*, where no stimulus at all is given, or to the method of right and wrong cases. The latter, making use as it does of the law of probability, not only requires an extremely large number of observations, but there is usually needed considerable preliminary and irregular experimentation in order to discover the conditions that will give a suitable proportion of right and wrong answers. The *Vexirversuch*, on the other hand, has never been systematized, and as it is usually introduced in an irregular fashion within the

method of minimal changes, it is apt to disturb the even tenor of the research, and disconcert the observer whenever he gets a hint of what is being done.

In the method of serial groups here proposed, the attempt is made to legitimate the 'catch' experiment, to introduce it as a continuous and regular element of the procedure, while securing certain advantages both of the method of minimal changes and of the method of right and wrong cases. To give a concrete illustration, suppose the following groups of experiments be carried out to determine the just perceptible extent of movement by sight, under the conditions described in the second part of the preceding paper:

Group I.			Group II.		
Exp. No.	Amount of Motion.	Judgment.	Exp. No.	Amount of Motion.	Judgment.
1	0	no motion.	11	0	no motion.
2	7 mm.	motion.	12	0	" "
3	7 mm.	"	13	6 mm.	motion.
4	7 mm.	no motion X.	14	6 mm.	"
5	0	" "	15	0	no motion.
6	0	" "	16	6 mm.	motion.
7	7 mm.	motion.	17	0	no motion.
8	0	no motion.	18	6 mm.	motion.
9	0	" "	19	0	slight motion X.
10	7 mm.	motion.	20	6 mm.	motion.

Group III.			Group IV.		
Exp. No.	Amount of Motion.	Judgment.	Exp. No.	Amount of Motion.	Judgment.
21	5 mm.	no motion X.	31	4 mm.	very slight motion.
22	0	" "	32	0	no motion.
23	5 mm.	motion.	33	0	" "
24	0	no motion.	34	0	very slight motion X.
25	5 mm.	" " X.	35	4 mm.	no motion X.
26	0	" "	36	0	" "
27	5 mm.	motion.	37	4 mm.	motion.
28	0	no motion.	38	4 mm.	"
29	0	" "	39	0	no motion.
30	5 mm.	motion.	40	4 mm.	" " X.

The X shows the errors in any group, and from these the threshold may be determined according to any proportion of correct and incorrect answers that may be chosen. In my own computations that group has been taken as giving the threshold beyond which less than eight out of the ten judgments are right. But a detail like this, as well as the exact number of

experiments that may best form a 'group,' might well be considered as subject to revision in the light of farther experience, and not as an essential part of the method. The essence of the matter is simply that there should be groups of experiments arranged in a regular series, the amount of positive stimulus, as one passes from group to group, being graduated according to the principle of the method of minimal changes; while within the limits of any one group a constant stimulus is irregularly alternated with cases where the stimulus is zero, thus uniting in the single group the basal principle of the method of right and wrong cases and that of the *Vexirversuch*. This may seem provokingly eclectic, but it is not exactly that; the different elements make an organic union, and not a mere patchwork. There is simply an attempt to make systematic what experimenters have frequently found themselves compelled to do in a casual and uncritical way.

One may perhaps repeat that this method is not proposed as a general substitute for the classic ones in use. It is well, however, to multiply our tools so that the best may be selected for the special work in hand. And this one has been found good for certain purposes, especially where suggestion plays a prominent rôle. The observer may here know from the very beginning the general method of procedure; he may know that zero-cases are to be irregularly alternated with those of positive stimulation, and his expectation is therefore less 'set' and influential. The zero-cases no longer come in as a kind of indignity upon the observer, as if his word were being questioned. The check here, because of its constancy, ceases to excite any feeling. The procedure, moreover, has the virtue of the method of minimal changes, in that the threshold is ascertained empirically, by actually crossing it. And while the principle of right and wrong cases is employed, with the powerful control which that always brings, yet there is no introduction of the intricate calculus of probability and a certain darkness that always shadows its results. It is true that the application of the method of serial groups is in a certain sense cumbersome, as compared with the method of minimal changes, since in a given time fewer determinations of the threshold can be obtained. But

with suitable rests between the 'groups,' there is no need of there being greater fatigue to the observer in the one case than in the other; and while the determinations may be fewer for the time expended, yet in most cases I have found that they more than make up in weight what they lack in number.

V. The Effect of Subdivisions on the Visual Estimate of Time.

BY MABEL LORENA NELSON.

It has been found by Dr. Ernst Meumann and others that the estimate of small time-intervals is influenced by the number of stimuli that fall within the interval. In the space illusion of sight, a single division of the standard will cause it to be underestimated, while more divisions will cause an overestimation; in touch, the effect of subdivisions depends on the absolute length of the standard.[1]

My object, in the following experiments, was to determine the effect of single and multiple divisions of the standard on times of longer duration than those investigated by Dr. Meumann, and to discover if there existed a temporal illusion comparable to the space illusions of sight and touch.

In Dr. Meumann's investigation of time intervals, he compares an 'empty' time — one bounded by two impressions — with times 'filled' with either three, five, six, nine or twelve impressions, inclusive of the terminal stimuli. His results [2] are, that for times from one tenth of a second to about four seconds, when the filled time comes first, the error in estimating is constantly positive — while for longer times the error is negative.

This seems to indicate that the effect of the filling is positive for the short times, and negative for the longer. The error found by Dr. Meumann is, however, not due to the filling alone, but is the result of two factors. It is generally conceded that even when two empty times are compared, there is a similar constant error, positive for short times, negative for longer.

[1] See the paper by Miss Alice Robertson, on "'Geometric-Optical' Illusions in Touch'" to be published subsequently.

[2] 'Beiträge zur Psychologie des Zeitbewusstseins,' *Phil. Studien*, XII., p. 127.

That there is a difference other than this constant error which must be attributed to the filling, Dr. Meumann shows—for, in those cases where the order is reversed, the empty time coming first, the sign of the error is also reversed — but the quantity of the error due to the filling alone he does not show, as these two factors are not quantitatively separated.

The longest period chosen by Dr. Meumann was nine seconds. The following experiments were taken to determine what effect the filling would have on longer periods, durations of several minutes.

The intervals chosen were one half, one, two, four, six and ten minutes; the filling, sensations of light.

Under each interval two sets of estimates were taken. The first, where the standard and compared times were both empty (marked *E–E* in the tables) was taken to determine the constant error due to the mere sequence of the two intervals. In the second set one of the times was always empty and the other filled (*E–F* and *F–E* in the tables). Any difference found between the estimates of the two sets, for a given interval, must be due to the filling.

The results as given in the tables are computed from five estimates under each interval for the empty time, and five for the filled. The average of the estimates is given; the difference between this and the standard interval, expressed as a per cent. of the standard; and the mean variation from the average estimate, expressed as a per cent. of the average. A second basis of comparison is the median of the five estimates and its difference, as a percentage, from the standard interval.

The effect due to the filling for each interval is found by subtracting the constant error, when both standard and compared times are empty, from the error in estimating when one of the times is filled. When the difference due to the filling in Tables I. and II. has a positive sign, it must be taken to mean that the filled time seemed shorter than an empty one of the same length. In Tables III., IV. and V., however, the order is reversed, the filled time being taken as the standard — a positive error here would indicate that the filled time seemed longer than the empty.

The method of taking the experiments was as follows: The subject sat in a darkened room before a screen and saw through an aperture in the screen, 5 mm. wide by 10 mm. in height, flashes of light through a noiseless pendulum behind. A flash of light marked the beginning of an empty time, a second flash its end. During the filled time the subject saw a flash of light every half second. There was in every experiment a pause of two seconds between the closing flash of the standard and the first flash of the compared time. The end of the compared time was marked off by a word from the subject when a time had elapsed which seemed to him equal to the standard.

In the first group of experiments the standard time was always empty. Two subjects were taken, *D* and *R*; the results are found in Tables I. and II. The great difficulty my subjects found, in the long intervals, in keeping their attention on the length of the standard, made it necessary to give them some idea of the interval to be used. Accordingly they were told whether the interval would be short (one half and one minute were called short), moderate (two and four minutes), or long (six and ten minutes), and whether the compared time would be filled or empty. This was, of course, in some respects a disadvantage, as it perhaps affected the lengths of the estimates, but as my object was to compare the estimates of a filled and an empty interval of time, the results are not invalidated by this guidance, as it was given alike in both sets.

In order that there might be no constant effect due to contrast, the order of using the different lengths as standards was not fixed, but was determined by chance. The time occupied by my work was never more than one hour at a time.

TABLE I.

SUBJECT *D*. *E–E*.

Interval.	Av. Estimate.	D %	M.V. %	Median.	D %	M.V. %
½ min.	28.2 secs.	— 6	27.5	34 secs.	+ 13	19.4
1 "	45.4 "	— 24.3	10.7	44 "	— 26.6	10.4
2 "	1 min. 40.6 "	— 16	12.6	1 min. 40 "	— 16.6	12.6
4 "	2 " 58.2 "	— 25.7	30.2	2 " 26 "	— 39.1	33.7
6 "	4 " 56.8 "	— 17	14.7	4 " 43 "	— 26.9	14.4
10 "	7 " 21.4 "	— 26.4	31	7 " 22 "	— 26.6	34.2

E–F.

½ min.	53.4 secs.	+ 78	40.1	43 secs.	+ 43.3	35.3
1 "	1 min. 18.6 "	+ 31	15.4	1 min. 17 "	+ 28.3	23.4
2 "	2 " 6.6 "	+ 5.5	30.4	1 " 55 "	— 4.1	35.1
4 "	4 " 6.8 "	+ 2.8	27.1	4 " 20 "	+ 8.3	27.7
6 "	6 " 7.4 "	+ 2	33.5	6 " 53 "	+ 14.7	27.5
10 "	7 " 1.8 "	— 29.7	18.4	7 " 26 "	— 25.6	16.3

Difference due to Filling.

	Average.	Median.
½ min.	+ 84 per cent.	+ 30 per cent.
1 "	+ 55.3 "	+ 54.9 "
2 "	+ 21.5 "	+ 12.5 "
4 "	+ 28.5 "	+ 47.4 "
6 "	+ 19 "	+ 41.6 "
10 "	— 3.3 "	+ 1 "

The results as given in Table I. show that when the average is taken as the basis of comparison, the effect of the filling on the estimates in the case of *D* is very constant. In the interval of a half minute, a filled time 84 per cent. greater than the empty is taken to be its equal. The effect of the filling seems to decrease as the length of the interval increases, until at ten minutes it is but little or nothing.

Table II.

Subject *R*. *E–E.*

Interval.	Av. Estimate.	D %	M.V. %	Median.	D %	M.V. %
½ min.	34.4 secs.	+ 14.6	26.2	34 secs.	+ 13	25.9
1 "	1 min. 12.2 "	+ 20.6	23.1	1 min. 13 "	+ 21.6	23.5
2 "	1 " 37.8 "	— 18.5	12.8	1 " 39 "	— 17.5	17.4
4 "	3 " 12.8 "	— 19.6	16.3	3 " 17 "	— 17.9	14.5
6 "	3 " 50.2 "	— 36	18.3	3 " 11 "	— 46.9	29.1
10 "	5 " 35.1 "	— 44	18	4 " 55 "	— 50.8	15.1

E–F.

½ min.	36.2 secs.	+ 20.6	33.5	40 secs.	+ 33.3	28.5
1 "	57.4 "	— 4.5	27.3	54 "	— 10	27.4
2 "	1 min. 47.4 "	— 10.5	29.8	1 min. 39 "	— 17.5	30.7
4 "	2 " 56.2 "	— 26.5	36.4	3 " 7 "	— 22	33.1
6 "	3 " 45 "	— 37.5	15.4	3 " 33 "	— 40.8	17.3
10 "	5 " 33 "	— 44.5	8.4	5 " 48 "	— 42	7.5

Difference due to Filling.

	Average.	Median.
½ min.	+ 6 per cent.	+20.3 per cent.
1 "	—25.1 "	—31.6 "
2 "	+ 8.5 "	0 "
4 "	— 6.9 "	— 4.1 "
6 "	— 1.5 "	+ 6.1 "
10 "	— .5 "	+ 8.8 "

With *R*, Table II., the filled time seems shorter than the empty in the intervals of one half and two minutes, but for all the other times it seems longer, taking the average as the basis of comparison. The error in the longest intervals is very small and its sign is changed when the median is taken instead of the average.

In continuing the experiments I introduced the filling into the first or standard time, the compared time being always empty. We should, therefore, expect a reversal of sign in the effect due to the filling. I found that after the former practice the subjects were able to hold their attention to the standard time without any foreknowledge as to its length. In this second group of experiments they were consequently ignorant of the character of the interval to be used. In all other respects the work was conducted exactly as before.

TABLE III.

SUBJECT *D*. *E–E.*

Interval.	Av. Estimate.	D %	M.V. %	Median.	D %	M.V. %
½ min.	30.8 secs.	+ 2.6	21.3	28 secs.	− 6.6	21.4
1 "	1 min. 10.2 "	+ 17	29.6	53 "	− 11.6	33.3
2 "	2 " 25 "	+ 20.8	16.6	2 mins. 10 "	+ 8.3	10
4 "	3 " 31.6 "	− 11.4	21.2	2 " 47 "	− 34.1	18.9
6 "	4 " 58.9 "	− 16.9	17.6	4 " 50 "	− 19.4	13.4
10 "	9 " 31.4 "	− 4.7	23.8	10 " 40 "	+ 6.6	22.2

F–E.

Interval.	Av. Estimate.	D %	M.V. %	Median.	D %	M.V. %
½ min.	35.1 secs.	+ 17	15.8	33 secs.	+ 10	13.6
1 "	55.4 "	− 7.6	29	46 "	− 26.6	26.5
2 "	1 min. 26.6 "	− 27.8	26.7	1 min. 14 "	− 38.3	22.1
4 "	3 " 53 "	− 2.9	18.9	4 " 19 "	+ 7.9	15.9
6 "	4 " 43.6 "	− 18.4	20.1	4 " 22 "	− 27.2	18.2
10 "	9 " 28.2 "	− 5.3	21.7	10 " 1 "	0	19.5

Difference due to Filling.

	Average.	Median.
½ min.	+ 14.4 per cent.	+ 16.6 per cent.
1 "	− 24.6 "	− 15 "
2 "	− 48.6 "	− 46.6 "
4 "	+ 8.5 "	+ 42 "
6 "	− 1.5 "	− 7.8 "
10 "	− .6 "	− 6.6 "

The results for *D*, Table III., show that with two exceptions (one half and four minutes) the difference due to the filling has

a negative sign, where in Table I. it was positive, indicating that in general the filled time seemed shorter than the empty.

TABLE IV.

SUBJECT *R*. *E–E*.

Interval.	Av. Estimate.	D %	M.V. %	Median.	D %	M.V. %
½ min.	28.2 secs.	— 6	27.7	23.5 secs.	—21.6	29.3
1 "	1 min. 10.8 "	+18	23.4	1 min. "	0	24.1
2 "	1 " 33.2 "	—25.5	24.3	1 " 28 "	—26.6	24.5
4 "	2 " 20.5 "	—41.5	25	2 " 14 "	—44.1	16.5
6 "	3 " 41.4 "	—38.5	19.2	3 " 56 "	—34.4	16.7
10 "	6 " 32.9 "	—34.5	25.7	5 " 54.5 "	—40.9	23.1

F–E.

Interval.	Av. Estimate.	D %	M.V. %	Median.	D %	M.V. %
½ min.	31.4 secs.	+ 4.6	30.3	37 secs.	+23.3	22.7
1 "	1 min. 4.7 "	+ 7.8	7.4	1 min. 2 "	+ 1.6	6.9
2 "	2 " 19.4 "	+16.1	29.2	2 " 20 "	+ 1.6	21.8
4 "	2 " 57.6 "	—26	28.2	2 " 34 "	—30.5	29.3
6 "	5 " 52 "	— 3.3	11.6	6 " 1 "	+ 0	10.7
10 "	6 " 15.4 "	—34.1	19.4	5 " 49 "	—41.8	19.3

Difference due to Filling.

	Average.	Median.
½ min.	+ 10.6 per cent.	+ 44.9 per cent.
1 "	— 10.2 "	+ 1.6 "
2 "	+ 41.6 "	+ 28.2 "
4 "	+ 15 "	+ 13.5 "
6 "	+ 35.2 "	+ 34.4 "
10 "	+ .4 "	— .9 "

Subject *R*, in Table IV., shows a positive difference, with the exception of the interval of one minute, where the median and average give conflicting results, and of the interval of ten minutes, where there is practically no effect.

We have in the tables four sets of figures that represent the effect of the filling on the estimates of *D* and of *R*. These figures are based on the average and the median of each of the two groups of experiments. I think we may safely infer that when the average and the median for any given interval of the same group have opposite signs, there is no clear effect due to the filling. The common result for these four modes of comparison would then be that the effect of the filling was to make the time seem shorter to *D* during the intervals of one, two, six and ten minutes. The two groups give conflicting results for the intervals of one half and four minutes, so that the position

of the filling — whether it came in the first or second of the intervals — was the more important factor. All four modes of comparison agree that to *R* the filled time seemed longer during all intervals except one half and one minute. In these two intervals the position of the filling is again the chief factor. In the case of the third subject, *S*, we have but one group of experiments. Here the filled time seemed longer at one minute but shorter at two, six and ten minutes. At one half and four minutes there seems to be no clear effect due to the filling.

It is evident from these results that the filling does not affect all three subjects alike. In general, the filled time seemed shorter than the empty to *D* and *S*, but longer to *R*, though there are exceptions in all three cases.

Table V.

Subject *S*. *E–E*.

Interval.	Av. Estimate.	D %	M.V. %	Median.	D %	M.V. %
½ min.	43.6 secs.	+ 43.6	20.4	40.5 secs.	+ 35	25.4
1 "	1 min. 6 "	+ 10	15.7	1 min. 2 "	+ 3.3	27.7
2 "	2 " 26.4 "	+ 22	27.7	2 " 47 "	+ 39.1	21.9
4 "	3 " 39.9 "	— 8.3	17.2	3 " 47 "	— 5.4	16.1
6 "	5 " 48.5 "	— 3.1	26.6	6 " 2 "	+ 3.3	24.8
10 "	10 " 10.6 "	+ 1.7	15.4	11 " 5 "	+ 10.8	5.9

F–E.

Interval.	Av. Estimate.	D %	M.V. %	Median.	D %	M.V. %
½ "	44.2 secs.	+ 47.3	21.3	39 secs.	+ 30	21.5
1 "	1 min. 40.9 "	+ 68.1	15.3	1 min. 44 "	+ 76.6	16
2 "	1 " 56 "	— 3.3	15.8	1 " 55 "	— 4.1	15.9
4 "	3 " 50.4 "	— 4	15.4	3 " 33.5 "	— 11.2	15
6 "	4 " 42.2 "	— 18.8	10.8	4 " 22 "	— 27.2	9.4
10 "	8 " 59.1 "	— 10.1	31.1	9 " 3.5 "	— 9.4	11.2

Difference due to Filling.

	Average.	Median.
½ min.	+ 3.7 per cent.	— 5 per cent.
1 "	+ 58.1 "	+ 73.3 "
2 "	— 25.3 "	— 43.2 "
4 "	+ 4.3 "	— 5.8 "
6 "	— 15.7 "	— 30.5 "
10 "	— 11.8 "	— 20.2 "

The estimates of empty times as compared with empty times, of the three subjects, as shown in Tables III., IV. and V., are in all respects comparable. If we consider only those intervals

where the average and the median are of the same sign, as de cisive, we have for *D* no apparent error at one half or one minute, a positive error at two minutes, a negative error at four and six minutes, and no error at ten minutes.

The results do not show a constant negative error, such as was found by Michael Ejner for intervals of one half, one, two, three and four minutes marked off by sound. I found in taking the estimates that when a short interval followed a longer one it was in general lengthened. This fact may in part account for the overestimation of the shorter intervals.

When we compare the estimates of empty times of *D* and *R* with those of the first group, Tables I. and II., we find that *D* has lengthened his estimates in this second group (compare Tables I. and II.). This change was not, I think, due to practice so much as to the increased strain of attention demanded by the lack of knowledge of the probable length of the standard. *R* has decreased the estimate of one half minute, and, in general, made the estimates of Table IV. smaller than those of Table II.

For all intervals longer than one or two minutes my subjects expressed a dissatisfaction with their estimates and felt that they made little, if any, difference between the longer intervals—all times seeming very long and very much alike. *R*, at times, could not *consciously* note any difference between standards of two and six minutes, or between those of four and ten minutes, even when they followed each other in close succession, though her *results* show a constant and decided difference. *D* had a better idea; for, when asked how long he thought an interval had been, his verbal answer more nearly approximated the duration he had just marked off as 'equal' in the experiment. *S* entered the experiments with a general knowledge of the lengths of time that were to be used as standards, though ignorant during the experiments as to what particular one was being given him—but beyond two minutes could not with any constancy identify them and tell whether the standards had been four, six or ten minutes.

During the longer periods it was impossible to keep the attention so closely fixed as during the intervals of one half and

one, or at most two minutes. It is at about this point that the change of sign occurs in the estimates. The general feeling of weariness seemed to be the chief criterion in the longer intervals.

The difference due to the filling was, I think, merely a difference in the direction of attention, the monotonous regularity of the lights being, in general, a means of holding the attention and preventing the mind from wandering. From this point of view the filled time was psychologically the more empty or barren of the two—the time being filled with monotonous sensations of light, but empty of vivid or interesting trains of thought. In looking back over it, then, there would be fewer changes in consciousness to remember, and hence the time would seem shorter. This would be in keeping with the fact that the increased mental activity produced by certain narcotic drugs makes time seem long; the person, on recovery, remembering the vast number of his experiences, overestimates the time.

What is the result of these experiments as compared with Dr. Meumann's, and with the space illusions of sight and touch? We find that in sight a space is overestimated when it is filled; an interrupted line will seem longer than a continuous one, a line divided into more than two parts longer than an undivided one of the same length. In touch, while an interrupted line of 10 mm. is underestimated, yet a longer one, 10 cm., will be overestimated when it is interrupted.[1] Whether the effect of the filling in these time intervals corresponds to that in the space illusions would depend on which of the two times we consider to be the filled; for, in these long intervals the sensations of light have but an indirect influence, and are not the only filling, nor the chief factor in the appreciation of time. Taking it, however, as ordinarily understood, we do not find here a constant negative error such as Dr. Meumann found in his longest times, although to two of the subjects, *D* and *S*, the filled time in general seemed shorter.

The results of the third subject, *R*, are more in accord with the space illusion of sight, and of touch when the standard is one of 10 cm.—the filling making the stretch seem longer.

[1] See the accompanying paper by Miss Robertson, already referred to.

To test the question as to the effect of single and multiple divisions of time, and to determine whether in the temporal estimate there was anything like the space error in vision, where halving produces a negative error and more divisions a positive one, visual intervals of 3, 6, 12, 18, 30 and 60 seconds were, by the same method as that described above, divided into halves, thirds and fourths.

Under each interval ten estimates were taken for an empty time, and ten for each character of filling, on each of two subjects. A pause of two seconds was made between the standard and the compared time, the compared time being here always empty. In order that the subjects might know when the end of the standard had arrived, the word 'now' was spoken immediately after the last flash of the standard. A stop-watch measuring two tenth seconds was used by the experimenter to mark off the estimate. Although there was a reaction error here, yet it was common to both sets of experiments alike, and so might be neglected in comparing them.

Table VI.

Subject *R.*

Interval.	No. of Lights.	Average.	M. V. %	Median.	M. V. %
3 secs.	2	3.94	10	4	9
	3	4.24	24	4	25
	4	4.71	15	4.7	14
6 "	2	6.28	16	6	16
	3	6.88	20	6.5	20
	4	8.82	16	8.4	16
	5	8.80	15	9.2	14
12 "	2	10.62	17	9.8	17
	3	10.96	13	10.4	10
	4	12.88	9	12.2	9
	5	13.38	8	13.8	7
18 "	2	13.24	16	13	15
	3	13.46	22	13.3	21
	4	16.44	13	17.3	12
	5	14.10	9	13.6	8
30 "	2	20.04	16	19.1	17
	3	20.22	23	19	20
	4	19.66	20	16.6	29
	5	24.58	26	23.9	22
60 "	2	32.84	26	28.7	26
	3	35.90	35	30.9	37
	4	39.76	19	40.1	19
	5	35.56	19	32.7	16

TABLE VII.

SUBJECT *Rd.*

Interval.	No. of Lights.	Average.	M. V. %	Median.	M. V. %
3 secs.	2	3.74	19	3.8	18
	3	4.56	13	4.5	13
	4	4.28	10	4.4	10
6 "	2	4.80	8	4.8	6
	3	6.68	17	6.9	16
	4	7.44	16	7.1	15
	5	8.36	12	8.7	12
12 "	2	10.07	14	10	14
	3	10.34	19	9.3	20
	4	11.11	22	9.9	22
	5	11.48	16	11.6	16
18 "	2	11.05	12	10.3	14
	3	13.83	11	14.35	9
	4	12.76	14	13.8	11
	5	15.35	12	15.3	10
30 "	2	17.41	14	16.3	14
	3	18.17	13	17.75	13
	4	17.99	18	16.35	16
	5	18.63	11	19.4	16
60 "	2	30.64	12	30.4	11
	3	34.56	16	34.3	17
	4	30.71	11	29.9	11
	5	33.54	17	34.2	16

The result we find (see Tables VI. and VII.) is that whether the average or the median be taken as the basis for comparison, the empty time seemed shorter than the filled, and, in general, the time seemed longer as the number of impressions was increased. For the three longer periods—eighteen, thirty and sixty seconds—the standard when divided into halves seemed longer than when divided into thirds to *Rd*; while to *R* the standard when divided into thirds seemed longer than when divided into fourths, during the intervals of eighteen and sixty seconds.

It is probable that in these longer periods the attention is not held closely to the sensations of light, so that other factors play a greater part in determining the estimate. In the shorter intervals, however, the mind can be kept relatively empty, so that the sensuous filling is the chief measure of duration. As long as the attention could be concentrated on the sensations, the number of lights would, I think, affect the estimate. Cases where in the present experiments the standard was subdivided

into thirds could always be consciously distinguished from those divided into halves, but many times where the standard was divided into fourths it could not be distinguished from thirds; more than fourths, I feel sure, could not have been apprehended without counting.

From these results we would say that in relatively short times as well as in spaces, the estimate is influenced by the number of impressions that fall within the stretch. There is no evidence whatever of a shortening up of the estimate due to the division of the standard into halves, such as is found in vision.

With *Rd* the division of the standard into fourths always gives a greater estimate than the division into thirds, but the estimate of thirds is often less than that of halves. *R*, with but three exceptions, increases the estimate as the number of divisions increases.

We find a great similarity in the absolute durations given in the estimate of the two subjects. Practice on longer intervals does not enable *R* to judge these shorter intervals any more accurately than *Rd*, who had no former practice, nor does it tend to reduce the variation; this being as large as in the former experiments and somewhat larger than that of *Rd*.

The position of the indifference point—where there is no absolute over- or underestimation—lies with both *R* and *Rd* between six and twelve seconds. The overestimation of three and six seconds may be due to assimilation with longer intervals. This would correspond with the results of Estel,[1] who found that an interval of three seconds, when it followed one of two seconds, seemed shorter than when it followed one of four seconds. This would also explain the shortening up of the estimates of empty intervals of thirty and sixty seconds in the case of subject *R* as compared with her former estimates given in Tables II. and IV.

As a final result of these experiments we find, in intervals of time ranging from three to sixty seconds, evidence of a temporal illusion very similar to the space illusion of sight. Both in time and in visual space, when there is more than a single division, the filled stretch is overestimated. We do not find,

[1] *Philosophische Studien*, II., p. 55.

however, that a single division shortens up the temporal estimate. This is in keeping with the space illusion of touch when the standard is 10 cm., but opposed to Dr. Meumann's results, as he finds intervals from four to nine seconds are underestimated when a single division is used.

As we increase the length of the standard interval to minutes, we do not get a direct reversal of the effect of the filling such as is found in touch; but we find the illusion either decreases or is entirely lost.

THE RELATION OF AUDITORY RHYTHM TO NERVOUS DISCHARGE.

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The term rhythm has commonly been employed in two acceptations. In its elementary signification the word describes a succession of events which follow one another with temporal uniformity; in sensory experience it is the periodic recurrence of a given stimulation; in connection with the sequences of auditory sensation it is the regular succession of sounds and silences. Such rhythms have been called *primary*. The second form, in contradistinction from the preceding, has been called *secondary rhythm*. It involves not only the regular recurrence of sensory stimuli but also a periodic differentiation among them. In this form of rhythm the unit is not a single element of sound but a group of such elements. Each of these unit groups is composed of all the sensory components lying between any one occurrence of the periodic differentiation and its proximal return.

Concerning the basis of this unification and segregation of groups theories have been advanced depending upon either of two factors, or upon their combination. It has been said that the essential principle of all rhythmical synthesis is to be sought in the temporal grouping of the sensations which support it. It has been asserted, on the contrary, that it is dependent solely upon the accentual variations whose recurrence characterizes the series. It has been urged, finally, that it is conditioned upon both of these aspects equally.[1] In the report of an experimental investigation on rhythm, published by the

[1] Theories of temporal synthesis: Hauptmann, Herbart, Lotze, Westphal. Theories of accentual synthesis: Guest, Tyrwhitt, Kostlin. Theories of interdependence: Meumann, Riemann.

writer in a monograph supplement of this periodical,[1] it was maintained that only the third of these hypotheses is consistent with the facts. No rhythm is presented by a series which perfectly fulfils the temporal conditions, so long as no recurrent accentual differentiation arises; and such recurrences of accent, on the other hand, are equally unable to support the impression of rhythm unless they fulfil specific temporal conditions in the form of their succession.

The elementary condition of the rhythm phenomenon is the periodic accentuation of an auditory succession occurring under specific temporal relations. While accentuation is essential to the appearance of rhythm, no specific mechanism is necessarily involved in its production. If forms of objective differentiation appear in the material, the variations which support the impression of rhythm may be indifferently of the intensity, quality or duration of the stimuli. Rhythm, therefore, does not consist of the presentation in consciousness of a regularly recurrent intensification of certain elements in a succession of auditory stimulations, for the impression is as distinct and adequate when the only differentiation presented by the series consists of a periodic lengthening of the component sounds or intervals. If the nature of the experience as a modification of consciousness be the same in these various cases, the essential constituents of the impression must be looked for elsewhere than in the special nature of the qualitative types of variation embodied in the sensory series, especially when there is taken into account also the fact that this impression is aroused when all such objective differences are absent, and the sole condition is observed that the stimuli follow one another at a certain rate.

The impression of rhythm depends, secondly, upon the repetition of the periodic differentiation manifested by the sensory series. That impression never arises from the presentation of a single differentiation period; it does appear at once and perfectly with its first recurrence. In other words, a simple measure cannot constitute a rhythmical sequence; two

[1] 'The Constitution of Simple Rhythm Forms.' Harvard Psychological Studies, Vol. I (Monograph Supplement 17); now in press.

such measures do so, and two measures form the simplest conceivable rhythm. In every concrete impression of rhythm two factors are involved: Firstly, a formal element which consists in an ideal scheme of relations which the sequence fulfils, and which contributes to the series of impressions a *Gestaltsqualität;* and, secondly, a material or time-filling element which consists in the repetition of functionally equivalent simple groups which constitute the structural elements of that ideal sequence. Recurrence is essential to the movement of the rhythm only, not to its form. The latter is apprehended immediately and completely upon a single recurrence of the rhythm group or period. As the rhythmical sequence advances the apprehension of form can be affected only by complication, that is, by the rise of new rhythmic forms into which the originally apprehended simple group enters as an elementary constituent. At each step the whole character of the rhythm is given up to that grade of synthesis which the total series presents. The simple measure, the verse, the stanza, and those poems which, like the sonnet, possess a distinct figuration are severally independent rhythmical forms, each of which is apprehended through a single presentation of its structure.

Such forms are not in themselves rhythms. The impression of rhythm arises only when these formal relations are embodied in a concrete movement. Formally adequate successions are constantly presented in our experience without arousing any impression of rhythm. Under certain circumstances the single occurrence of a rhythmically related group of sounds may give rise to the impression of rhythm, namely, when the melody which contains it is familiar, or the mind is pervaded by a tendency to rhythmical expression of this particular form. In such cases, however, the simple measure does not contain the rhythm, but only initiates a process the continuance of which supports the experience.

In the third place, the impression of rhythm arises only under the maintenance of specific temporal conditions in the succession of sensory stimuli which support it. Though the superior and inferior limits of the absolute rates of succession which condition the feeling of rhythm in a series of sounds

have been variously reported by different observers, these divergences presuppose in all cases the indubitable existence of such limits. Intensive subordination among the elements of the objective series and periodicity in their recurrence are by themselves wholly insufficient to give rise to the feeling of rhythm. Were the impression dependent solely upon a perception of the maintenance of such relations in a series of auditory stimulations, the temporal limits to the experience which are actually encountered would be inconceivable. We do thus conceptually speak of geological rhythms, but this is a figurative projection of the term into realms which our experience can never penetrate. One may conceive the existence of a consciousness upon which such a series of stimulations would produce an impression of rhythm, but for the human consciousness it is forever unrhythmical, and no amount of effort at conceiving it in this way will avail to give one the peculiar experience of 'feeling a rhythm'. in it. That experience is as new and different from the apprehension of the isolated elements of a slower series as the continuous musical tone produced by a siren is different from the succession of puffs which one hears when the mechanism revolves at a slower rate.

The facts involved in the elementary conditions of the rhythm impression which have important theoretic bearing may then be stated shortly as follows: First, the process of accentuation is not necessarily connected with any specific type of objective change and may arise in their absence. It must therefore be an activity which these objective factors occasion only and do not contain, and its appearance is most closely connected with the temporal relations embodied in the series. Second, the scheme of a rhythm group, in its temporal and intensive relations, gives only the formal conditions for the appearance of the rhythm impression. For its realization the accentuation of certain elements of the series must be periodically renewed, and this repetition of functionally integrated groups persists through all grades of structural synthesis. Third, of all the possible rates of succession which a series of auditory sensations may present the feeling of rhythm accompanies those only which fall within a narrowly limited range.

Above and below these limits the succession of stimuli fails to support the experience. It cannot there be mediated either through the intensification of objective accentual differences, nor by reinforcement through any type of subjective emphasis.

Upon the intimate nature of the process of rhythmical accentuation in its relation to the phenomena of temporal limits must turn our theories as to the nature and fundamental conditions of the rhythm experience. In the analysis of this process it is important to discriminate purely rhythmic factors from concomitant but independent elements of experience. To the latter class belong all complications and enrichment of the bare limiting stimuli upon which the definition of periodicity depends. It is necessary, above all, to inquire by what means the rhythm experience is superinduced upon the presentation of stimuli which do not manifest those differences with which its arousal is ordinarily connected. The existence of such contributed or subjective forms makes it necessary to take into account the possibility of its presence as a transforming agency in every experience of rhythm, objective as well as subjective. If it create rhythm where none previously existed, it may complicate and enrich simple rhythm forms produced by the voice or instrument which affords the sensory material of experience. In point of fact this subjective contribution is not restricted to series from which intensive and temporal differences have been eliminated. It appears equally in the organization of recurrent forms which possess a distinct objective configuration. Under specific conditions of temporal succession such identically repeated forms — for example, successions of rapidly uttered dactyls — are subjectively differentiated into alternate groups having the relation of major and minor phases, in which both temporal and intensive values are quantitatively unlike for the apprehension of the hearer. Upon this constant subtle accompaniment the characteristic form and affective overtone of the rhythm impression in some degree depend, and it is at least conceivable that the essential element of the experience should lie in the subjective contribution itself—that rhythm is never a fact of perception alone, but essentially involves an active attitude on the part of the apperceiving subject. This is the conception to

which the writer was led in the course of the investigation already referred to, and the point of view may be stated in a few words. *Rhythm is always produced.* The bare auditory perception of a series of sounds, the uniformity of which is broken by periodic reinforcements, no more affords the peculiar experience of rhythm than does the perception of those visual symbols which represent the relations of such a series of sounds in musical notation. The successive stimulations must start a series of motor impulses somewhere before its rhythm is felt. Apart from such a pulse of bodily change the perception of a rhythmical series of sounds would be the bare abstract apprehension of their varying intensities and intervals.

The fundamental conditions of the rhythm experience are therefore to be looked for in the laws of periodicity of functioning in the bodily organism. It is because these processes take place under conditions of regularly recurrent change that the time element becomes important in rhythmical sequences. The existence of beautifully proportioned durations in rhythm is a purely derivative fact. We do not take pleasure in such series because of the proportion or simplicity of their relations, but because their patterns are reproductions of the natural forms of our own activities. The pleasure which rhythm affords arises from the coincidence of subjective and objective change.

In regard to the nature of the mechanism involved in this process of subjective activity, with the phases of which the elements of objective stimulation must be specifically coördinated in order that the impression of rhythm shall arise, we may conceive, in the first place, a periodical facilitation and inhibition of nervous activity to arise from the relation between the periodicity of its own rhythm of functioning and certain intervals in the objective series of stimulations. If such a physiological rhythm appears in the functioning of the central nervous system, a periodic increase and decrease should occur in the intensity of the sensations coördinated with a series of unchanging stimulations, according as its elements were correlated with positive or negative phases of the nervous activity. The effect would be analogous to that consequent upon successive tensions and relaxations in the drum of the ear, and the rhythm must be

called, in so far as the sensory experience is concerned, an objective rhythm, illusion arising only in connection with the interpretation of the source of the positive changes which appear in consciousness. In the second place, we may conceive the succession of auditory stimulations to arouse a parallel motor accompaniment in the form of sensation reflexes occurring in some part of the bodily organism. The impression of rhythm under this conception is due to a system of kinæsthetic sensations whereby periodic elements of the primary auditory series are reinforced in such a way that the whole sum of sensational material rhythmically increases and decreases. This active accompaniment may be conceived to take the form either of an accentuation of certain members of the series only, or of a continuous accompaniment in which a reaction is coördinated with every element of sensation, the violence of the motor discharge periodically increasing at those points in the series which form the successive accents. Here, also, there is present in consciousness a real rhythmical series, but it is the accompaniment, not the original sequence of sensations, which is thus characterized. Both of these relations between the rhythmically repeated stimulation and the nervous activity, namely, functional facilitation and reflex motor discharge, I conceive to be represented in the conditions which support the impression of rhythm. The sole existence of the former type of effect is the theoretic limit of a process which in its actual occurrence always involves elements of the latter kind.

The components of this rhythmical accompaniment present wide variations. The muscular contractions which mediate it are to be looked for among the most mobile members of the body. The tongue, the head, the jaws, the fingers and the feet are visibly employed in keeping time to rhythmical stimuli. Of greater prevalence but much more difficult of observation are contractions giving rise to sensations of strain in the throat, head, chest and limbs, tensions in the vocal and respiratory muscles, and above all the simultaneous innervation of opposed sets of extensor and flexor muscles producing alternate phases of rigidity and relaxation which do not affect the local relations of the organ in which they take place. In the apprehension of

rhythm forms this kinæsthetic comment appears ineradicable. For each individual the inhibition of the natural set of motor accompaniments interferes with the impression of the rhythm. In many cases when these expressive movements are eliminated the sense of rhythm is lost. The process of voluntary inhibition of particular modes of apprehending rhythmical material consists in breaking up the special set of motor emphases which supports that form of perception. The endeavor to apprehend the sensory sequence under a different rhythm form is uniformly accompanied by the motor emphasis of those accentual points which are characteristic of the novel rhythm. In the attempt to perceive as monotonous an undifferentiated series of sounds which has hitherto supported a subjective rhythm the recurrent motor accentuations of the previously dominant rhythm are got rid of, not by a suppression of all accompaniment, but by the equal emphasis of each element as it appears. Finally, the voluntary inhibition of such sensation reflexes is a process which affects only a limited number of factors; it cannot be carried out in any completeness. Sufficient rhythmical accompaniment may in all cases escape notice to give definiteness and character to the rhythm.

This process of secondary motor response is not necessarily confined to the voluntary muscle groups. It may be reflected into the mechanisms of accommodation in the organs of perception through the periodical renewal of sensorial attention. The effort to attend to a sensory stimulus involves, as part of the concomitant organic adjustment, an adaptive change in the condition of the sensory apparatus which increases the sensitiveness of its response to incoming stimuli. Every return of the attitude of attention to auditory stimulation temporarily sharpens the sense of hearing by drawing to a nicer tension the membranes of the drum of the ear. In such a case we have to do with a process which is not itself the object of voluntary adjustment but the secondary result of such an activity. For periodicity in the recurrence of such an attitude we may look to either of the two sources already stated, a physiological rhythm in the nervous mechanism of sense perception itself, whereby the tonicity of the organ suffers regularly recurrent changes, or an

adaptive process concomitant with the rhythmical motor accompaniment of the sensory series, those elements which are emphasized being likewise more closely attended to, and thereby receiving reinforcement as sensation intensities. The pulses of attention in the apprehension of rhythm are at least not more rapid than the recurrence of the simple group, for such structural units are without exception apperceived as unities and not as a succession of separable parts.

In the rhythmization of undifferentiated auditory material the relation of the processes of sensory accommodation and motor innervation to the stimulation series becomes of paramount importance. In it, while the rhythmical form is a purely subjective contribution, the illusion of objective differentiation is complete. Subjective rhythm appears only under temporal relations of narrower range than those which condition objective forms, but within these limits it arises spontaneously. It is not, however, strictly or uniformly uncontrollable; the establishment of such a form of apprehension does not necessarily involve its perpetuation during the continuance of the auditory series. In certain cases the mere attending to the apparent rhythm causes its disappearance. When of a more integrated type the rhythm is commonly suppressed if each element be regarded in isolation from the succession of which it forms a part. When attention drifts away and the series of sounds — or as much as can be grasped in a single act of attention — is regarded as a whole, the rhythmic differentiation reappears. In more obstinate cases the method of breaking up the rhythm is to emphasize, by a more or less violent reaction, those elements which fall into unaccented positions in the series or to emphasize equally each sound as it appears. The rhythmic apprehension of undifferentiated material is also subject to frequent fluctuations both in its form and continuity. It appears, undergoes change of structure, is dissolved and reappears within relatively short periods of time. When the experience is supported by the conception of an ideal form which the series of stimuli fulfills, the apprehension of the material in a single mode may be indefinitely prolonged. In the experience of rhythm, then, whether supported by a succession of sensa-

tions presenting figured groups, or by a uniform series, there is presented a process of apprehension penetrated at every point by secondary motor impulses.

It will help us to understand the way in which this motor accompaniment is aroused in the presence of a rhythmical series of sounds if we recall the primitive relations of reaction to stimulation. In concise form it may thus be stated: Every presentation tends to arouse some movement. In kind this movement is imitative of the original. A succession of regularly recurrent stimuli, therefore, tends to set up a process of rhythmical movement. Not to accompany the presentation of a stimulus in this way indicates inhibition of some sort.

But this is not an explanation of the tendency to prefer one type of movement to another, to imitate a rhythmical but not an unrhythmical series of stimulations. The unrhythmical succession is vastly more frequent in our experience than the rhythmical. If the establishment of particular types of organized movement depended upon the frequency with which the like relations were presented in the world of our objective experience, we should prefer forms of irregular movement. The actual tendency, on the contrary, is toward the embodiment of rhythmical relations in our movements; we maintain a tempo, we beat time, we accompany a rhythmical series of sounds but not an irregularly recurring series. The relation to experience is not that of the establishment of a simple correspondence of subjective habit with objective conditions.

There is present in such processes a factor which is reinforced by stimuli occurring in periodic succession but which is unsupported, or inhibited, by unrhythmical series. This is the law of nervous action in virtue of which the form of a movement once originated tends to be perpetuated. The kinæsthetic sensation aroused by the perception-reflex, or imitative reaction, is itself a presentation having the nature of an incitement to the repetition of the movement. It partakes of the character of the original stimulus which provoked the primary reaction, and tends to bring about again a discharge into the same complex of muscles. Thus, in a nervous system uncomplicated by other simultaneously active processes, the origina-

tion of any movement tends toward the establishment of a rhythmical series of reactions by the reciprocal arousal of movement and kinæsthetic sensation within a single closed arc.

This primitive condition of activity is disturbed, in the organism possessed of a highly developed nervous system, both through the interference of intense outward stimulations occurring at irregular intervals, and by counter-suggestions to action of a conflicting type where ideal associations are present. Even in such organisms when these factors of complication are withdrawn the process may spin itself out indefinitely. This dissociation of the motor process from the control of ideal associations takes place under both normal and pathological conditions. In the normal subject it is presented in states of inattention to those minor tensions and reactions of the body which are either mechanized or unconnected with the purposeful activity of the moment. Such are the rhythmical lilting, thrumming and beating of time into which the mobile fingers and tongue fall during moods of idleness or abstraction, and the larger innervation processes of the body which have grown thoroughly habitual, such as the series of reactions by which the process of walking is carried on. The same sequence of relations is presented, in connection with a simpler type of associative system, in the habit of young children endlessly to repeat single sounds or imitative movements.

The pathological forms of this process are many. It appears during the transient suspensions of rational control occurring in fever delirium, in the meaningless repetition by the patient of a word or syllable which he has uttered or heard. It is presented in the tetanic innervation of the muscular system characteristic of catalepsy, and is probably represented also in the continued reproduction of a suggested movement by the hypnotic subject until the process is arrested by the hypnotizer. It is manifested in more pronounced and obstinate forms among idiots and the insane, where its exaggerated and persistent types have led to the coining of the descriptive term 'echolalia.' I cannot agree with Professor Ziehen's extension of a pleasure valence to the origin of these reactions, though accepting his first statement without reserve when he says: "As regards the

succession of sensations, therefore, a regular periodicity is the chief condition for the appearance of feelings of pleasure. It is not mere chance that maniacs and those afflicted with emotional paranoia often speak in rhythm and rhyme. Such phenomena harmonize rather with the morbid, positive emotional states characterizing these forms of psychosis."[1] These are forms of activity to be explained not by the concept of a deliberate pleasure-seeking but as the expression of a primitive, uncontrollable impulse to utterance, a reversion to a simpler type of activity, in which the elementary rhythms of motor innervation are uncomplicated by a richly developed system of associated ideas.[2]

The general conditions underlying all these phenomena are characterized by simplicity or primitiveness in comparison with the workings of the normal mature nervous system — conditions in which the sensori-motor arc manifesting activity is in relative functional isolation, such that the interferences with its characteristic forms of innervation by other simultaneously active brain processes are reduced to a minimum. Primitively it needs not a regular succession of sound stimulations, an objective rhythm, to establish a rhythmical series of movements; a single originating stimulus, if it be sufficient to bring about the initial reaction, will thus serve to set up a succession of repetitions. The fortune of this series of reverberations does not depend solely upon the type of functioning represented by the nervous arc within which the reciprocal movements are conceived to take place. It is subject to influences both from beyond the organism and from within it, either of which may tend toward reinforcement, on the one hand, or inhibition, on the other; and upon the relation of the sum of these extraneous factors to the arc primarily excited the subsequent course of the series of reactions depends.

[1] Ziehen: 'Physiological Psychology,' p. 148.

[2] This is characteristic also of the spontaneous rhythmization of rapidly succeeding homogeneous sounds, of which one observer in the experiments already referred to writes: "I certainly never try to do it; and, so far as I can see, it is never prompted by any desire to get satisfaction. It seems to be a spontaneous act, which when it arises causes satisfaction in a very slight degree."

Reinforcement of the primitive tendency to repetition may come either from a regularly repeated external stimulation,[1] or from the idea of a series of rhythmical movements to be carried out. The process set up by a single stimulation is unlikely, under any actual set of conditions, to establish successfully a system of rhythmical movements. The probabilities are immeasurably in favor of its being lost in the complicated set of independent sensational and ideational activities with which the general system is throbbing. When, however, the original stimulation is regularly renewed, the waning process receives periodical reinforcement by positive increments of stimulation and the reactive motor accompaniment waxes from moment to moment until a fully developed set of rhythmical movements is established. Under this conception the character of the motor accompaniment, as also the intensity of the rhythm feeling, should undergo a certain growth. The former should not appear either so vigorous or so exactly coördinated in the first phases of response to the recurrent stimulation as in its later forms. We should expect it to be susceptible also of indefinite exaggeration until conceivably the diffusion of the wave of motor response involved the whole body in violent changes. As a matter of fact we know from common observation that the tendency to accompany an auditory rhythm by movements of the bodily members, by humming, lilting and singing in tune, by beating time with finger, or foot, or head, by waving the arms, swaying the body, etc., does not arise at once upon the initial stimulation, nor is it at first fully developed. It manifests a period of latency and a process of growth, requiring an appreciable time to attain its maximum. One can observe this

[1] Within the category of such external stimulations we must of course include the secondary sensations, of sight and sound, which accompany the kinæsthetic feelings as common products of the movements involved in beating out a rhythm. The exactness and perpetuation of the rhythm habit depend upon the nature of these derivatives of the motor reaction, as well as upon the resident sensations themselves. In proportion as these limiting stimuli become distinct and forcible does their value as incentives to the repetition of the reaction increase. Sensations of movement, kinæsthetic and visual, of resistance, and of sound all combine to reinforce the intensity of the rhythm activity; and it is probable that the most effective union of resident with secondary sensations is attained in the process of vocal utterance.

reinforcement in the manner in which an audience accompanies a piece of music by gradually increasing movement until the whole roomful of persons is swaying and beating to the rhythm like a single instrument.

One may mark its persistence, too, in the difficulty met with in the attempt to break up such a system of rhythmical movements when once thoroughly set agoing. The obstinate running through the head, even to a pitch of distress, of a tune which one has heard, the habits of strumming and lilting which establish themselves as the reflex accompaniments of the return of certain attitudes, and which cannot be shaken off, are examples of this tendency. We should include within the same category the fact that the subjective rhythm which appears when uniform sounds follow one another at certain intervals of time does not manifest itself at once on the inception of the series, as is the case with the earliest measures of an objective rhythm, but develops slowly, the appearance of this contributed rhythm being dependent, as has been already said, upon the development of a rhythmical process of discharge within the motor centers. As the concomitant of such a developing system of reactions within the organism we should look for an increase in the affective over-tone, and this mounting rhythm feeling, with its undeniable pleasurable quality, is doubtless itself a positive factor in accentuating and perpetuating the process.

We should also be prepared to find a development of sensitiveness to rhythmical relations, manifested in a progress toward uniformity among the successive reactions and a refinement of perception in regard to irregularities occurring within the series of stimulations. Both of these forms of change appear in the results of the experimental investigation already referred to. On the one hand, both unit measures and larger series of reactions show a progressive integration, manifested in the reduction of the indices of mean variation, and on the other, the threshold of just discernible variation from type of structure is similarly lowered as the series of rhythmical sounds advances toward its close.

It is obvious that in the setting up of such an organic rhythmical accompaniment the temporal relation of the series of stimu-

lations to the nervous habit of the organism must be a factor of great importance. If there is to be objective reinforcement of an organic tendency to reproduce the movement, the phases of the two processes must coincide, or at least approach within the limits of adjustment on the part of the organism. The elements of a regularly recurrent stimulation may be conceived to exert an inhibitive influence equally well with any incidental or irregular form of excitation, if its periodic phases are in opposition to those of the natural rhythm of the sensori-motor process itself. Similarly, it will altogether fail to support the tendency if its recurrences are so infrequent that the impulse to reaction has died out before the following stimulation takes place. What particular intervals between successive stimuli are most favorable to the establishment of the process is a matter to be determined empirically by direct experimentation; but apart from the actual determination of the values of such limits we should expect that the tendency to rhythmize indifferent sensory stimuli, or to accompany them by rhythmical movements, will depend upon the relation of their absolute rate of succession to the natural rhythm of the bodily processes involved.

That such limits exist has long been established; it requires but slight observation to discover them. Below a certain rate they fail to arouse any involuntary response; between this and a superior limit the tendency to segregate and accentuate the material and to accompany it by motor reactions is practically uncontrollable; beyond this upper threshold the organic adjustments fail to follow the accelerated series, though here, under certain conditions, a new form of rhythmization may supplant the old, the impulses proceeding not from the single elements of stimulation serially but from the integration of successive groups which now replace those elements as units.

The rhythm activity, then, represents a relatively undifferentiated type of reaction. Its appearance as a spontaneous exercise and as a reflex accompaniment is a manifestation of the primitive tendency to reaction toward presented objects, and of an equally primitive tendency to perpetuate a movement once made. It belongs to a class of activities which we habitually connect with early ages of development and with the lower parts of the

nervous system. The opportunity for the outcropping of these primitive activities is presented under conditions in which the higher brain processes are inhibited, or reduced below their normal preponderance. In such temporary moods of the normal subject as revery and abstraction, in the lack of occupation and of mental strenuousness, we should look for conditions favorable to the development of spontaneous or reflex rhythm activities. Now it is just in such moments of relaxation and mental drifting, when consciousness is occupied with no serious or pressing duty, that the impulsive rhythm habit, humming, tapping, singing, and the like, and the reflex habit of accompanying any objective series of regularly recurrent stimulations, characteristically crop out. The active association of ideas is either suppressed or drafted off into an entirely different set of centers than those concerned in the production of the rhythmical reactions. We are either mentally vacant at the moment, and at the mercy of vagrant stimuli, or we are absorbed in some mental process which makes no tax on the periphery of the body, and are usually unaware at the moment of the rhythmical reaction process which, thus accidentally originated, is spinning itself out in the absence of inhibition from the higher centers of the brain.

Further, the persistent and exaggerated types of rhythmical motor activity presented in the abnormal conditions already referred to confirm this view of the relation of the rhythm habit to the general activity of consciousness. In fever delirium the processes of orderly association characteristic of normal life, by which random meaningless movements are inhibited, is interrupted, the primary reaction centers of the lower brain are released from the inhibitive control of the cortex, and the utterance or other movement, once originated, tends to a prolonged repetition, limited only by the final exhaustion of the muscular or nervous system. Similar conditions are presented in the tendency of children and idiots to prolong rhythmical repetitions of movements and to rhyme over sounds to themselves. The prevalence of the element of rhythm in primitive music and ceremonial is indicative of a lower stage of development than our civilized type. An indispensable condition of the

purity of the rhythm activity is its independence of a changing ideational content. The most perfect rhythm, whether simple repetition of a motor reaction at regular intervals, or the complex grouping of such rhythmic material into synthetic units, is that which is most free from secondary significance. Its movement is inevitably clogged when it is made a form in which to cast discursive thought. The most adequate rhythms are those which present only a pure sensory stimulus to the motor process, uncharged with symbolic value, which allow the attention to center in the reciprocal play within the sensori-motor arc, and make no draft upon it by a stream of changing ideas which the limiting stimuli are made to suggest. Its purest and most effective embodiment is found in such sequences as that of the drum-tap, the most potent incitement to the rhythm habit of all forms of auditory stimulation which have been invented. Add any new element to this perfect instrument and its effectiveness in arousing the feeling and evoking the accompaniment of rhythm is proportionally weakened. Introduce change of pitch and give melody to the series of sounds; combine the beating of the skin with the tones of vibrating strings, or pulsating air, or quivering reeds, in chords, harmonies and orchestral effects; above all, make the series of sounds which sustain the rhythm the vehicle for a train of mental images of objects and relations, with their ever-changing influence upon the direction of the attention process, and the rhythm is thereby impaired, proportionally to the capacity of the factors of melody, harmony, or rational significance to attract attention to themselves and away from the pure rhythmical element. "Almost every civilized country has its national melodies, in the form of songs, dances, or marches. While most of these melodies have remained local, some of them, particularly dances, have been taken up by the great masters and introduced into general musical literature; and though many of the original melodies have long been forgotten and changed, yet the form and characteristic elements of them have remained.

"The great masters who employed certain national dance-forms for special compositions, or introduced them in their greater works, although describing the general distinctions of

these dances, did not adhere strictly enough to the more detailed characteristics; and handled the form with such individual freedom that it became under their hands an artistic dance-form, but ceased to be a dance in the popular acceptation.

"Thus, the dances of Bach; the minuets of Hayden, Mozart, and Beethoven; the waltzes and polonaises of Schubert; the mazurkas, waltzes, and polonaises of Chopin, etc., are artistic forms; and not intended for practical dance purposes."[1]

The transposition and complication of accents which these art-forms involve disguise their primary rhythms, and make difficult or impossible the coördination with them of the rhythmic phases of movement upon which the dance depends. Dominant and effective rhythm can exist only in simple musical and poetical compositions. It is not in these compositions that the most highly differentiated and complexly organized types of rhythm are to be found; but in them the rhythm factor is greater relatively to the whole production. The person in whom discrimination of pitch-differences and melodic or harmonic relations is poor, and for whom the coördination of groups of tones is difficult, finds his enjoyment of musical performances in the pleasure of the immediate rhythm experience. When this is overlaid by those more complicated types of coordination which delight the trained musician's ear, his pleasure vanishes, for the composition then produces no distinct effect upon him, it gives no incentive to rhythmical accompaniment and appears only as a confused medley of sound. One gifted with a musical ear or trained in the appreciation of musical effects, who follows and enjoys such a composition, does not find his pleasure simply in a rhythm which is of a more synthetic type than his untrained neighbor is able to grasp. His attention is primarily attracted to other elements, to the sweep of melodic phrasing, the antiphony of passage to passage, and the larger unities of composition, which a rhythmical undertone of recurrent accentuation and analogy of form enables him conceptually to grasp. He does not feel the rhythm primitively and immediately as the other does that of his tattoo upon the table or his meaningless lilting; it does not provoke in him any dis-

[1] Christiani, A. F.: 'The Principles of Expression,' etc., pp. 93-95.

cernible type of motor response; it has lost its intrinsic significance for the æsthetic subject in whose consciousness it appears, and serves chiefly as a secondary means of extending the scope of the attention process in its effort to grasp an ever wider set of pleasurable stimuli.

Still more strikingly, when the rhythmical sounds are clothed with ideas, and the sensations which support the rhythm are at the same time the symbols of rational speech, does the association of these secondary factors with the fundamental element of the rhythm phenomenon interfere with the purity and power of its effect. Poetry, like the opera, is an irrational combination of two processes, either of which, for its free activity, presupposes the absence of the other. Rational thought, fit sequence of ideas, demands a free movement among complicated elements in which no precise periodic return to the same origin is conceivable; while just this condition of a never-ceasing repetition of similar forms of relationship is fundamental in every rhythm process. For the rhythm experience is one and the same in music and in verse, the vehicle of its expression alone differing in the two cases. The proportionate relations of rhythm are more exactly fulfilled in musical performances than in poetical recitation; for the material of music, as a set of symbols, is incomparably less definite than that of verse. The draft upon the attention in the former case leads constantly back to the immediate relations of quality, intensity and duration existing in the sensuous material itself. There is no parallel series of logically connected images, as in the case of poetry, to distract the mind from the changes in the sensory material by which they have been suggested. And the conditions of musical expression make it imperative to observe the laws of rhythm, since upon these depend in great measure the processes of comparison and coördination which are superimposed upon the immediate enjoyment of the sensory qualities which the music presents.

In poetic expression the function of rhythm is essentially more subordinate, then, than in that of music. The rhythm of poetry pulls in one direction; the thought it presents, in another. The most perfect of rhythmic verse-forms is found in the recita-

tion of nursery rhymes and nonsense jingles, where the reciter is least seduced from the production of a beautifully proportioned series of recurrent groups of motor reactions by the associations of vitally significant words. The high relative value of the rhythmical element in these compositions is not due to any unusual consonance between the orthographical and the rhythmical accents. On the contrary, as Sanford has pointed out, no other popular form of poetic writing makes such audacious demands upon the rhythm sense of its reader. Rests, elisions, prolongations, even transpositions of orthographical accent are freely made, yet the ordinary reader is carried securely through their complex changes. This is so because the maintenance of the formal key-note given by the verse structure and initial orthographic accents is rendered easy by the simplicity of their content as compared with other forms of poetical writing.

In proportion as the structure of the verse-form grows more complex, or the burden of thought more exacting in the demands which it makes upon the attention, this sense of rhythm fades. Only in those pieces in which there is a return of the thought upon itself periodically in the form of a refrain, or the meaningless repetitions (meaningless from the point of view of associative thought, but most significant in their relation to the function of rhythm) of folk-songs and nursery rhymes, can the words of the verse be said to reinforce the rhythm in any way—that is, only when, at the sacrifice of their intrinsic significance, the words are used as practically pure sensory rhythm elements. It is because of the disjunction between sounds as the material of rhythm and sounds as the symbols of rational speech, and their more specific correlation in the former case with a parallel set of motor innervations, that one has a more distinct feeling of rhythm in the spoken verse of a tongue with which one is unfamiliar than in that of one's own speech; that one best clarifies the rhythmic structure of a stanza by substituting for the significant words of the poet a lilting utterance of meaningless syllables; and that the 'music' of a poem may persistently haunt one, while the words and even the thought and general topic with which it is concerned are wholly irrecoverable.

In the relation of the successive sounds of a rhythmical sequence to a coördinated system of motor impulses is finally to be sought an understanding of the constitution of objective rhythm forms and the laws of their synthesis. These phenomena include the characteristic distribution of intensities within the unit group, the apparent accentuation of an element which arises from the simple extension of the interval following it, the exaggeration of the duration of the accented element and its following interval, and the concomitant variation of intervals in dependence upon the intensive magnitude of preceding elements. In connection with uttered rhythms our knowledge of the laws under which nervous discharges take place—such as the relation of intensity in the innervation to the temporal curves of latent period, contraction and release—makes possible the prediction of certain relations of stress and duration among the elements of the group. Under this concept the alternating phases of expectation and its fulfilment which characterize the experience of rhythm will be resolved into psychological attitudes which are physiologically conditioned by systems of strains and releases due to the rhythmical opposition of those phases of motor innervation which the reaction accompaniment involves. Not only in connection with uttered rhythms, but also in regard to the forms in which objective series are apprehended, this concept should find application; for in the latter case the same characteristic relations appear between stress and duration as in the former. These connections cannot be due to differences in the course of the sensory after-images resulting from intensively varying stimuli, for the causal relations between the two may be inverted, that sound which is prolonged or followed by a prolonged interval receiving apparent accentuation. Their conditions are to be sought rather in the system of motor discharges which the series of stimuli arouses, especially in its relation to the rhythmical renewal of sensorial attention and the temporal curve of the nervous changes involved in the mechanism of accommodation.

DISCUSSION AND REPORTS.

THE RELATIONS OF FEELING AND ATTENTION.

In Professor Pillsbury's review of Zoneff and Meumann's article 'Ueber Begleiterscheinungen psychischer Vorgänge in Athem und Puls, I., (PSYCH. REV., IX., 405) occurs the following paragraph:

Another statement of the text-books, founded upon self-observation, which was investigated was the effect of directing the attention to the feeling process itself. The usual statement that attention to the feeling destroys the feeling was not found to be true of the effect upon the physiological concomitants. On the contrary, the effects upon both respiration and pulse are increased. The authors however state that they think the result would have been different if psychological introspection had been attempted instead of mere attention to the feeling.

These sentences seem to me to be misleading, both as regards the text-book statements and as regards the new results. Since the point involved is systematically important, I venture to offer the following criticisms.

1. Professor Pillsbury draws a distinction between text-book statements, based on self-observation, and the experimental results of Zoneff and Meumann. It need hardly be said that the latter are also based upon self-observations. "Der Reagent wurde angewiesen, sich einmal auf den Reiz, * * * ein anderes Mal auf das Gefühl zu concentriren" (*Phil. Stud.*, XVIII., 67). The conclusions drawn by the two authors are avowedly gathered from the introspective reports of the observers. The reviewer's meaning must, therefore, be that the text-book statements are based upon casual introspections, made without experimental control. I have been guilty of 'the usual statement' in two text-books, and may be allowed, perhaps, to fit the cap to my own head. If I do this, I must emphatically repudiate Professor Pillsbury's suggestion. I have made a large number of experiments myself, and have taken introspective records from many other observers under experimental conditions. As long ago as 1894 I published a brief summary of experiments upon the question (*Phil. Rev.*, III., 431; the systematic setting of the paper is crude, but the observations are reliable). And I have no doubt that the other writers whom the reviewer has in mind also wrote from first-hand evidence.

2. It is, however, more important to look at the new experimental evidence. Zoneff and Meumann found that their results showed discrepancies, which they explain as follows. "Concentration of the attention upon a mental process" may mean two different things. It may mean, in the first place, that "one keeps the feeling, by voluntary effort, for a relatively long time at the conscious point of regard, and analyzes it. One reflects, *e. g.*, whether the feeling is more or less pleasant (or unpleasant). In this case there is a real analysis of the feeling, accompanied by a certain bodily tension. With *this* sort of self-observation of mental states, the changes of breathing and pulse must, very evidently, be those of attention." It may mean, secondly, that "the observer tries to bring the feeling as well as may be to *consciousness*, *without however analyzing it.* In other words, the feeling makes its way to the conscious point of regard, *but that is all; nothing more is done with it.* The feeling is, so to say, felt resignedly" (*loc. cit.*, 73: italics in the original). Both possibilities were realized in the experiments: "diese beiden Möglichkeiten kommen bei unseren Versuchen vor und deshalb diese scheinbare Resultatlosigkeit in der Tabelle" (*ibid.*). There is no word of the authors' thinking that "the result would have been different if psychological introspection had been attempted." The results in the two cases *were* different.

Zoneff and Meumann conclude that "a mere direction of attention upon the feeling intensifies it; if, on the other hand, the feeling is made the object of a psychological analysis and in this sense the object of attention, it is considerably weakened, indeed, even entirely destroyed" (*ibid.*). The first clause summarizes the facts (*a*) that certain observers, when called upon to 'attend to the feeling,' interpreted the requirement to mean, 'Let the feeling have its own way with you,' 'Resign yourself passively to it,' and (*b*) that, under these conditions, the feeling attained its fullest intensity. The second clause repeats 'the usual statement' of the text-books. But there is, surely, no contradiction. Nor is there, in the first clause, anything new. The rules for affective work given in my Experimental Psychology, I., i., 98 f.; I., ii., 151 f., 166, 181 f., tally exactly with Zoneff and Meumann's results.

The only possible point of dispute, therefore, between the two investigators and the text-books would seem to be a question of terms. When you tell an observer to attend to his feeling, and he resigns himself passively and absorbedly to it, can his mental state properly be described as a 'blosse Richtung der Aufmerksamkeit auf das

Gefühl'? One may say, I think, that his interpretation of the order is perfectly natural and justifiable. He instinctively takes up that mental attitude which favors the feeling at the expense of the other contents of consciousness. One must also say, however, that the interpretation is naïve. With more introspective experience, he gives a narrower meaning to his instructions; he purposely abstracts from sensible processes, and tries actively to attend to the affective contents as such. "It may be remarked further," say Zoneff and Meumann, "that the better an observer is practised in self-observation the more does he incline to analyze the feeling, and not to enjoy it or thrust it from him" (*ibid.*). Now Zoneff and Meumann are themselves taking a non-committal position as regards systematic questions (*cf.* Meumann's Remarks on Terminology, *loc. cit.*, 2). They may, therefore, for the time being, accept the naïve observer's interpretation at its face value, and classify his reactions as instances of 'simple attention to feeling.' But a psychology must go farther. How does a 'mere direction of the attention upon' a process differ from an 'analytic attention' to it? Why should the two forms or modes of attention lead to diametrically opposed affective results? I have committed myself to the view that attention, in the first case, is a passive attention to the sensible substrate of the feeling, while in the second case it is a baffled or misdirected active attention. The view is tentative, but it has at least the advantage of setting the two groups of facts in systematic relation. And I see no reason, in anything that they have so far written, why Zoneff and Meumann should not accept it.

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ON McDOUGALL'S OBSERVATIONS REGARDING LIGHT- AND COLOR-VISION.

Mr. W. McDougall published in the January, April and July numbers of *Mind*, 1901, three papers entitled: 'Some New Observations in Support of Thomas Young's Theory of Light and Colour Vision.' Mr. McDougall there described some fifty experiments which he had made, and drew certain conclusions from them. On the basis of these observations he would reject the Hering-Müller theory of light- and color-vision and substitute for it a modified form of Thomas Young's theory. Mr. McDougall's observations present an extensive array of ingenious devices and of suggestive facts, but in the interpretation of such an amount of material it is perhaps inevitable that some

few points should be called in question. The purpose of this comment is to suggest:

(*a*) That these experiments do not in every particular warrant the conclusions drawn from them.

(*b*) That there are certain facts which offer difficulties for the Young-McDougall theory.

(*c*) A general reason why the Hering-Müller type of theory is preferable to the other.

Mr. McDougall's position is outlined in what follows. He begins with the remark, 'that all writers assume that the physiological and psychological processes in the visual areas of the cortex run exactly parallel to retinal processes.' What can such a statement mean? Is not every act of selective attention among visual stimuli a case where cortical and retinal processes are not exactly parallel? Are not visual reflexes one kind of instance, and subliminal stimulations another, in which peripheral excitation and consciousness are disparate? Yet what is commoner or more obvious? What does Mr. McDougall suppose that psychologists mean by the distinction of central and peripheral? Surely this difference is not a new observation. However, this disparity of cerebral and retinal process is illustrated at some length, and this is, he says, the basis of his theory.

On the retinal side Mr. McDougall assumes that there are four exciting substances, which he calls the red, green, blue and white (R, G, B, and W) *x*-substances. When light acts upon the retina it frees (by decomposition) these substances, and by a further chemical process (combination) they each excite their corresponding nerve endings, that is, the termini of four distinct color-systems, R, G, B and W cerebro-retinal systems. Light of any color sets free all four kinds of substances but acts most vigorously on one; thus R light frees B, G and W, but it frees R in greater quantity, hence the predominance of red sensation. These substances are highly diffusible, so that when they are freed in any definite part of the retina they tend to diffuse themselves into surrounding areas and there give rise to sensations.

This is the entire retinal basis for the phenomena of light and color vision; all variations not referable to these processes must be explained in cerebral terms. Thus yellow sensation is a psychical fusion of red and green, and the experience black is the inhibition or lack of sensation.

The particular point at which the Hering theory (and Müller's as well) is attacked is in the discussion of the facts of simultaneous and successive contrast and induction. The case of white light is ex-

plained first. In *simultaneous induction* the directly stimulated area of the retina diffuses the x-substances into adjoining parts and they there give rise to sensations. *Successive induction* is the persistence of white x-substances in those areas. In *simultaneous contrast* the cortical processes excited by a W patch of light tend to inhibit all those from the rest of the visual field. *Successive contrast* is the case where the cortical processes excited by x-substances diffused into outside areas come to predominate over those aroused by the directly stimulated part, that is, where the after-image appears as a white halo around a black area.

Black sensation is in each case the result of cortical inhibition.

He then goes on to explain all contrasts and after-images of colored lights in terms of the cortex. The principle underlying color-contrast is that W being the resultant of the simultaneous activity of the R, G, B color-systems, the inhibition of W by W involves the inhibition of R by R, B by B and of G by G. Now if the retina be stimulated by a patch of red on a gray background, the R tends much more strongly to inhibit the R in the gray ground than the B, G, which the patch of red contains tends to inhibit the B, G of the ground, hence the effect of simultaneous contrast. Successive contrast, or the case of the complementarily colored after-image, is explained by cortical changes, and simultaneous and successive induction or colored light are closely analogous to those phenomena in the case of white light.

Inhibition with Mr. McDougall means always cortical inhibition, but for R, G, B and W he allows a retinal origin which he denies to black. This brings us to the principal difference between the Hering and McDougall theories, which is that Hering assigns a special retinal process as the correlative of the sensation black, whereas Mr. McDougall maintains that no such independent process exists and that the physiological basis of black sensation is simply cortical inhibition.

In support of his general position Mr. McDougall makes a very interesting and exhaustive study of all forms of retinal rivalry, and he states most ably the important part played by the cortex in light- and color-vision. In connection with details of his theory, however, he adduces, among others, the following experiments:

1. See Observations 16, 17, 18 and 19, Article I. The discussion here is whether the relation of black to white is the same as the relation of other colors to their complementaries in the matter of successive contrast or the negative after-image. The first observation was made on a disc white at its center and shading gradually out to black against a black ground. After fixating this disc of shaded black and

white no negative after-image, only a positive one, was seen. After this colored discs constructed in the same way were used, for example, a disc with a blue center shading gradually out to black. In this case a negative after-image was observed. This experiment is taken as evidence that black cannot excite an after-image of complementary tone in the way that other colors excite their complementaries, but it is obvious that for such a demonstration this experiment cannot be correctly organized. Mr. McDougall is comparing the reactions of different pairs of colors, and if, as the first member of the comparison, he takes a disc shading by many gradations between the complementaries white and black, he ought logically to take as his second case a disc, say, of blue gradually shading into yellow, or of red merging by degrees into green. If he wants to compare black with other colors he must take them under like conditions. For the purpose of such a comparison it does not have the slightest point to contrast black shading up to white, with blue shading off to black.

2. Mr. McDougall wishes to prove that the after-image and the direct image have their seat in the same part of the cerebro-retinal system. He fixated for a few seconds a patch of red light (Obs. 9) and then planted its green after-image beside the red patch. The red and green then faded and reappeared together. This proves, he says, that they are affected in the same way by the same factors, and that the seat of the after-image is in the retina.

A little farther on (Obs. 14) Mr. McDougall showed that an after-image may be inhibited by a direct image on a different part of the same retina. Why not say that this proves that the two are not affected in the same way by the same factors, and that the after-image is, therefore, not in the retina? It seems curious that Mr. McDougall should not bring in other available evidence if he thinks the retinal location of the after-image requires proof. He might quote not only the experimental results of Exner, but certain striking facts of common observation, *e. g.*, that the after-image follows every movement of the eye-ball.

3. Yellow sensation, he holds, is due to the psychical fusion of red and green. This is proved by the facts: (*a*) That the mixture of red and green give a yellow of good saturation (Obs. 49); (*b*) that the cycle of color in the after-image of yellow shows markedly a red and a green phase (Obs. 41). But what does Mr. McDougall say to the fact that in peripheral color-vision we can see yellow with elements of the retina which are absolutely insensitive to the difference of red and green?

In this connection he quotes Mrs. Franklin's criticism on the theories of Gegenfarben, in which she says that the red and green, which subjectively are most saturated, are not complementary (antagonistic), but that they produce yellow. But this observation gives no countenance to a psychic fusion theory of yellow, since we have in Mrs. Franklin's own words a more plausible explanation in retinal terms: "Blue and yellow are complementary colors, but red and green, when acting in conjunction, re-compose the yellow-producing substance out of which they have been developed, instead of together making white" (PSYCHOLOGICAL REVIEW, January, 1899).

4. On the basis of one experiment (Obs. 22) Mr. McDougall disputes the fact of the simultaneous contrast effect of black. Against this position we may quote the results of Hering, Mach, Sherrington and others. In the following simple test it seems scarcely possible that one should fail to get this effect.

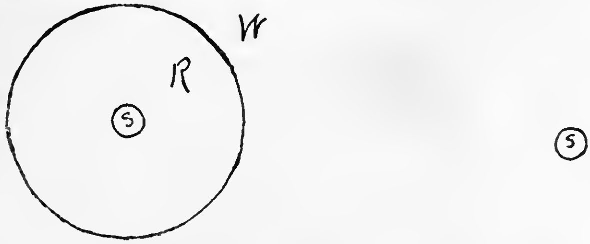

FIG. 1.

On a white ground (Fig. 1) lies a circular red patch three centimeters in diameter with a black spot at the center. Fixate the black spot for five or six seconds and the red will begin to show dark at the edges and bright red immediately around the spot, while between the dark and the bright red there still lies a broad ring of the unaffected color. Now transfer the after-image to a white ground and center it on a black spot like the first one. In a few seconds a rim of brilliant white light appears to radiate from around the black spot.

5. In the case of the binocular combination of color Mr. McDougall rests in absolute security. Here the fusion seems unquestionably to be a psychic one; but in view of the strong sympathetic connection, upon which he dwells, between the cerebro-retinal systems of the two eyes, is there not a retinal explanation at least possible even here? We may conceive that the blue which excites one retina finds a slight response in the other, while the red which is acting on the second retina is echoed in the first, so that the resulting purple is after all based upon retinal fusion.

6. In observation 24 there is reproduced an experiment of Müller's, in which dark red against a black ground is compared with the same red against a white ground.

FIG. 2.

Upon rotating the two discs (Fig. 2) the red of *A* appears more saturated than the red of *B*. One would suppose that the result might be explained as a mixture of red with contrast white in disc *A* or a mixture of red with contrast black in disc *B*; but Mr. McDougall rejects such a view, without giving any definite reason, and he says that in disk *B* the white of the background inhibits the white which the red light contains and hence makes that red still darker.

Many of these experiments are extremely interesting and valuable in themselves, but one could scarcely agree that they support unequivocally any theory which has been advanced in these papers. In fact the conclusions seem often a trifle premature and the above citations serve to illustrate that some of them are logically a little surprising.

If Mr. McDougall is to maintain the theory that there is no black-exciting process in the retina, but that black is the inhibition or absence of sensation, he has still one or two questions to answer:

1. If contrast black is the result of suppression of faint gray light by a bright white light, that is, if it is a case of cortical inhibition, why does it persist when we expressly attend to that part of the field in which it appears, why does the faint gray light not emerge into consciousness?

2. If black is mere lack of sensation, why do we not see black with the blind spot in monocular vision?

3. If black is a negative element, why does a mixture of black with other colors change not only their saturation, but their color-tone as well.

4. Subjectively, black is not a 'neutral' color, nor is gray a mere diminution of white; gray appears to us something qualitatively very different from white, like the mixture of some positive element with white. The following observation may touch this point: In mixing

complementaries on the color-wheel, if the colors differ in luminosity so that the flicker is pronounced, *e. g.*, in blue and yellow, it is noticed that the field suddenly assumes a rough resemblance to a black-and-white checker-board. Instead of the two colors changing at once into an even medium gray the process of mixture takes on a very decided black-and-white phase, suggesting that the resulting gray is due to the stimulation of two distinct retinal elements.

The tendency throughout Mr. McDougall's papers is to emphasize the part played in light- and color-vision by the cortex. Whenever possible he explains in terms of cortical attention, and he apparently feels that it is a commendable simplicity in his theory to assume as few retinal processes as possible and to refer all further variations to purely central fusions and suppressions.

Attention seems to be the refuge from a great many psychological tangles, since there is nothing which it cannot be made to explain. The fluctuation of attention is the very basis of our psychic life, it is the fundamental problem which holds the others in solution, and it is just on this account that it ought to be avoided as an explanation. No one has a right to resort to an ultimate principle until all hope of a more special explanation has failed. The tendency of scientific theory is to pass from the vague generality to the specific cause, and in dealing with sense-discrimination it has moved steadily away from the explanation in cortical terms towards that in peripheral terms. It is a more exact and intelligible mode of thought to conceive of sense differences as determined by structural differences in the sense-organs, than to suppose that they are perceived by some deft and mysterious intelligence in the cortex. Although no general considerations of this sort can prove or disprove any particular theory, yet they can lay out the direction of progress for that theory.

In comparing the retinal theories of Hering, Müller, Franklin, with the psychical of Young-McDougall, we may say that the former are forward-moving theories and the latter is a step backward. Mr. McDougall's method may be thoroughly safe, but it will never be illuminating. The Hering theory as a type is in the line of progress, and although there may be difficulties with its precise formulation it must still be the preferable hypothesis.

Mr. McDougall's criticisms have a positive value in searching so minutely as they do the frailties of the Hering theory, and in reminding psychologists of the inadequacy of any color-theory which has yet been proposed.

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SOME CHARACTERISTICS OF THE GENETIC METHOD.[1]

It may be readily confessed that the following observations arose in response to a personal want and interest. While engaged in a wider topic, the inquiries failed to discover any thoroughgoing presentation and discussion of this particular theme by psychologists. The conception of genesis has become overshadowing to all types of current science. The adaptation of method to this conception has been attempted repeatedly, and sketches of the character of the result in method have been given here and there, but not in a thoroughly satisfying manner. These reflections are not set forth as answering all the demands of that personal want and interest, but rather with the hope that they encompass the field with some system and penetration.

Our inquiry, then, is not to be merely a census among psychological methods, but rather how best psychology can create its problems, and how it can be sponsor for true problems of science. As soon as a science ventures beyond a dogmatic attitude—and to be dogmatic is not an impossible quality for science!—it is immediately confronted with the question of method. By method, we must understand the fullest sense of the term. It is not the mere pedagogical preliminary, which almost every text-book and other exposition of the science implies that it is. These well-nigh invariably thrust the treatment of method into the foreground. All this involves the assumption that method is a very minor matter, being only the question of procedure, which can be described and settled once and for all, and then immediately forgotten.

The usual text-book notion of method in psychology regards it as something quite splitable. There are the direct and the indirect methods; the subjective and the objective; the individual and the collective; the logical and the genetic; the experimental and the enumerative; the analytical and the synthetical; the historical and the projective; human and comparative—and there might be more of them if only our language were rich enough in adjectives! These methods are such, however, as it is often said, only in light of the data which are the peculiar property of psychology—just as though a fact of soul life is so completely and so variously accessible to the gaze of the inquirer! Or, these varieties of method are also taken to mean that no single soul fact recurs in the acquisitions of any two or more methods, but appears only once, and in the net returns of but one given method—

[1] Read by title before the Tenth Annual Meeting of the American Psychological Association, December, 1901, University of Chicago.

just as though the sum of methods should be regarded as the sum of soul facts! Undoubtedly there is a valuable suggestion as to the well-nigh inexhaustible richness of mental experience in this array of methods as forms of procedure, all of which are to be applied before the student can lay claim to possessing anything more than a mere outline or elementary scheme of a reasoned account of 'inner experience.' If psychology has shown more or less surely that the soul can be gotten at by such an indefinite number of modes of approach, that fact, as a mere item in human civilization, is worth all the multitudinous efforts required to establish it. At the same time, there is not the most credulous of us moderns who for a moment believes that a grab-baggy, drag-netty *potpourri* ever constitutes a science. To lay bare the soul, to expose its intimate transactions, is the business of life, whether in the career of the individual, the activity of a pupil in the school, or the issues of the hero in the scheme of art and literature. Science may represent life, but does not manufacture it. If not, then the completest and minutest biography would be the acme of the science of the soul.

If these strictures upon current notions of method—and they apply *mutatis mutandis* to every other type of science besides that of psychology—be true and fitting, we may find here an antithesis which may well venture to become an antinomy in our most desperate efforts to force psychological science to square itself with both method of thought and content of experience in thorough and unquestioned conclusion as to what our real knowledge of soul life shall be.

The truer conception of scientific method, while not denying its more limited pedagogical function as just indicated, requires that we conceive the question of method as almost identical with the question of the standpoint, yea, of the science itself. In no field of modern inquiry is this larger conception more appropriate to the nature of the subject-matter and more indicative of the scientific scope than in the case of psychology. This truth has often been inadvertently admitted in the many early discussions about 'introspection,' where the question of method confessedly became a question of the possibility of the science. With all our marvellous advances in instrumentation and tabulation, the very continuity of psychology as a widening field of human activity is still dependent upon the alternative which grew out of those discussions: either introspect, or give up the science. The standpoint of interpretation, however, stands or falls with this outcome, all of which is summed up in the final question: How are we going *to think* in psychology?

This does not necessarily imply that there is a mathematical thinking, a physical thinking, a chemical thinking, a biological thinking, each of which, or all of which, may be different from psychological thinking. For, all science is produced by human thought, in fact *is* human thought. And yet, the psychologist in mathematics, for example, presents the spectacle of the proverbial bull in the china shop! Even so is it with the chemist in psychology, or with any other form of 'cross-breeding' among the departments of science and the devotees of science. Apperception, both scientifically and psychologically, may thus appear to be a very demon, tormenting us with all the incredulities and monstrosities of an integral science.

The first suggestion arising from our topic is twofold: first, that method is an external feature of science—its means, which may be extremely variable, to an end of verified knowledge of given or natural objects; and second, that there may be more than one method in psychological science. These two suggestions are at bottom one and the same, namely, that manual dexterity and the thinking which enter into the constitution of a science are radically different. This implies that any sort of a skill in attacking an unknown region of facts might unquestionably go along in company with any sort of an acceptable theoretical construction of such facts. It undermines the notion that the specific scientist himself should be fundamentally interested in and concerned with the formulation of the method which he is to 'apply.' It is in contradiction with the actual experiences of psychologists, and fails to throw any light upon the progress of psychology, whether its standpoint has been metaphysical or more modernly positivistic. It disregards that trait of investigation which is expressed in the necessity of dealing with facts after they have once been elevated out of the *milieu* of experience, which, to be consequential, must be present in the very beginning of the methodical procedure. Thus, and as could well be indicated in other ways, this suggestion is barren of fruit in offering a clue to the proper regard of the relation of attitude and the execution of scientific thought. This is particularly the case in psychology, where no sort of subterfuge in attacking the domain of conscious facts, whether by mere enumeration, or reaction sorting, or brain division, or concept perfection, can by any possible means bring about the conquering leap over the walls of experience which have been assailed by all the indefinite variety of machines devised for the warfare of scientific method. Thus far our considerations have been negative, leaving upon our hands the strong intimation that there is apparent reason for a continual and basal cleavage between psychological science and its method.

Coming to our topic, we may ask, what is the 'genetic' method? What are its *differentiæ?* How does it get 'applied'? What does it take for granted? What are its results? We can probably find no uniform opinion among the psychologists as to what the method really is. And, if we turn to the uses which have been made and are being made of it, our inquiry would probably be rewarded with a discovery of still greater divergence among them. Of the fact that there is a widening dependence upon this method, one can readily be assured by a hasty glance at any one of the various psychological indexes which are available annually, or at the end of every text-book chapter written now-a-days. This disparity of usage and the persistency of mutual criticism among the methods produces a feeling of despair, which finds some relief, at least, if no definite solution, by dashing off a host of questions which come trooping up as soon as it is proposed to state what the method is. What is 'genesis' anyway? What can we mean by 'psychological' genesis? Is genesis more a matter of time and history, or of analysis and logic? Can the genetic standpoint ever delimit itself with any sort of analytical precision? If one can speak of mental genesis, how is this to be distinguished from physical genesis? Why is it incumbent upon psychology to go outside of consciousness in order to get a scientific, explanatory principle? Can the genetic method offer any valid basis for the classification of data? Whence the push which is said to be revealed in mental 'unfolding'? Can the method recognize any 'content' in consciousness? Is it not compelled to disregard everything except processes—presuming, of course, that there might be anything else in the mind? How and why does the soul make itself to become what it is? Or, does something else make the soul as we find it to be? Is there anything immanent in soul growth? These questions, it is true, are not usually put in these terms, and often they are not mentioned at all as representing the real problem of the genetic method. Yet they may be regarded as offering real aspects of the great problem to the solution of which we may conceive the method as setting itself.

To be in accord with the spirit of its application, one would be compelled to believe that the genetic method is but a dawning of the latest researches. To be true to the historic facts, however, one finds that the birth of the science lay in the application of the method itself —or, at least of *a* genetic method—to the facts of soul life. Both the science and the method had their coördinate beginnings with Aristotle's attempt to approach the soul of man. If we also look at the modern era (without attempting to be exhaustive and systematic), we

see that Waitz in 1852 conceived of psychology as both comparative and genetic. The next year Morrell depicted stages of development among the 'faculties' themselves. In 1855, Spencer's attempt with the method became a supreme application of it, with all fidelity to its peculiar logic. Twenty years later Volkmann wrote his two splendid volumes 'from the standpoint of realism according to the genetic method.' Preyer worked industriously from the morphological point of view. Or, within the last decade, Ladd makes an application of the method to the specific forms of mental life in man. Stout attempts to apply the method on the ground that 'the individual consciousness is but a fragment of the general system of the world,' and Baldwin sets off the method as pursuing distinct problems after the work of analysis seems to have been done.[1]

The historical potency of the problem of psychogenesis as determining the dominant method of psychology would be radically missed, aside from the bare historical data of the appearance of the concept in scientific consciousness, if no attention were paid to the introduction and use of the term 'function' in all discussions pertaining to consciousness (not to the physiological issues). The Herbartian dethronement of the 'faculty' concept made way for the more modern aspirant of 'function' (which was, indeed, vigorously promoted by Herbart's own use of *Vorstellung*). Indeed, the term 'function' implies the genetic method; and perhaps it is the great achievement of this standpoint to have focused attention upon function as an object of analysis which could hopefully resolve the confusion imminent in all treatment of mind and brain. Lewes was probably too conciliatory for real genetic progress, when he suggested that 'function' should mean 'endowment,' and 'faculty,' 'variation.' Steadily since, however, function is the psychological slogan of such warriors as Ward, James and Stout. One biologist recently so far forgot the genesis of this conception of science as to affirm unblushingly that consciousness as a function came before there was ever an organized brain! Thus one can sketch the wide-spreading, silent, and at times uncertain intrusion of the genetic spirit into our science.

We approach the intimate nature of the genetic method if we turn to notice first, as one of its peculiar characteristics, the material out of which it proposes to build the science of mind and the material of consciousness which is to receive recognition from its hand. It pre-

[1] These and later references are not to be regarded as 'demonstrations' aiding the examination; they are mentioned more as conservative instances, and not as types indicative of variations in the method.

supposes the work of analysis as being more or less completed, and in part on the basis of these results attempts to build up its own constructions. It is extremely selective in accepting the material with which it works. Not *all* soul facts are employed in working out its great generalizations. It endeavors to account for all the functions of consciousness known to the method of analysis, by referring them to so-called lower grades of processes. In dealing with the lowest 'forms' of conscious action, such as 'feeling,' 'instinct,' automatic processes, etc., genetic psychology violently differentiates itself from the other methods. That material of psychology which bears the marks of being the most persistent, ubiquitous, and hence most 'fundamental' in the economy of the soul as a product of nature, is said to be the special field in which this method plies its industry.

Closely connected with this feature of material is the dominant conception which overshadows its elaboration. When we regard the soul as a 'thing,' the method of deduction is alone appropriate to meet the demands of both science and its material. When we regard the soul as a 'process,' the inductive method alone can be adapted to the content of that experience to which all psychology should in every case refer. It is only as we combine these views of the soul that we have brought ourselves into a position where the question of genesis is said to force itself paramountly upon our attention. It is only when we think of the soul as in a process of development, 'unfoldment,' if one please, or even as that development itself and nothing more or less, that the genetic method is made possible, with all its reconstructive force in the sciences which are vitally concerned in displaying truth about that soul life. The 'process' and the 'thing' views must be unified in a conception that regards the thing not as static, nor the process as merely drifting; then we first come to a right view concerning the object of psychological research. Extremists, however, readily contend that the genetic method can accept neither the thing nor the process view of the soul, and that they are mutually exclusive. And, indeed, it is just against this 'misuse' of the genetic method that we would offer our solemn protest.

The remaining question is that of *how* we shall regard the soul as developing: whether individually, cerebrally, or racially. No single discussion can settle this dispute, which will go on indefinitely. But it can at least put its finger upon the position this issue holds with respect to the general features of the method as a whole. But here method is seen again to mean essentially the principles of interpretation.

Other features of the genetic method appear, secondly, if we compare it with two or three generic lines of current psychological thought. It apparently tends to do away with brain psychology. In fact, it would seem at first blush that the inheritance of soul portions, a confessed feature of mind according to the method of genesis, could be described irrespective of the empirical relations the soul may sustain to cerebral structure and function. The fair hope of a consciousness psychology seems now almost fully within our grasp. But this is only the first impression of the relation between a brain and a genetic psychology. For, no sooner have the genetic functions been sorted out in consciousness than the reconstruction of them into the working processes in consciousness must trace their progressive synthesis up through the aggregation of ganglia, which finally produce the brain. Whether the genetic method can ever get beyond this point of view, and be able consistently to regard consciousness as a 'natural' object, yet somehow not totally dependent upon the brain, is a very serious and difficult question. To the biologist, of course, such a question means nothing. His point of view demands nothing more than cell colonization!

There is also a very positive way in which the genetic method is but brain psychology thinly disguised. For, cerebral morphology is explored, as well as comparative morphology, as containing the infallible transcript of the order of psychological precedence. Definite organ, so the credo would run, stands for definite function. All this would have to be taken with a meaning much stronger than that of a mere analogical parallelism.

When contrasted with the stimulus psychology—which is probably more fundamental from the standpoint of collective science than any other form of objective psychology, and especially more so than a brain psychology because of its extreme approximation to accuracy in the possibilities of measuring in definite terms the causal quantities of stimuli—the genetic psychology differentiates itself both by its mode of 'explanation' and by the material of the soul life which it accepts as needing explanation. It is one of the unique features of science that there is a logical connection between its methods and the materials which are accredited for study; and this comes out in this present contrast. Stimulus psychology attempts to account for the origin of the content of the individual mind by going directly to physical nature; genetic psychology struggles with the material which can be connected with ancestral genesis.

One of the most interesting aspects of the genetic method appears

in a farther contrast of it with the experimental method. Here we have, indeed, the two progressive rivals for the control of the entire field of psychology. The earlier claimant is that of genesis; but the more widely applied is that of experiment. The latter, too, has enriched the content of known truths about the soul and conditions of its activity more largely than the former. This is due, of course, to the fact that experiment is generously taken to include not only a stimulus, but an enumeration psychology. The weakness of the genetic, and the superiority of the experimental method are at once seen in those attempts at a use of the latter in the interest of the former. No *single* experiment and no complicated series of *experiments* can ever succeed in demonstrating the fact of genesis or in picking out any given stage in its processes. For, an experiment gives us a 'dead' result which, by its own method of ascertainment, cannot be woven into an elementary connection with genetic data. Geneticists may endeavor to employ experiment; but this offers 'results' only when the particular hypothesis of development has been first elaborated. Comparison with a special intent alone can turn the data of experiment into support for the hypothesis. This limitation of the genetic method, and this antithesis between these two types of method, are even more marked if the subject of experimental reaction is selected from the animals or from the period of infancy. Thus it is made clear that the genetic method is primarily not a method of obtaining data and validating judgments about them, but a method of inference in construing such and, perhaps, all other data into an ideal scheme.

As a result of all these features, the genetic method comes forward with its characteristic claim that it alone does entirely away with the radical distinction between the method and the content of a science. From the standpoint of genesis, these are to be identified. The order of exposition is to be the order of development, and the order of development is to be the order of exposition and of scientific discovery. The progress of thought on the way to knowledge is to repeat the progress which in fact nature presents in this case, the 'psychological' aspects of all nature. Here, then, is the supposed unity: the development is the science, and the science is the development.

Method, however, in this application first means mere order of development, and a truthful arrangement in thought is held to be that which copies the earlier arrangement which nature exhibits. This attempt at unification is also less a feature of method, than it is a matter of postulation. Here we find one of the limitations of the genetic method, and also one of its good features, which, namely, meets in a

unique way our earlier demand that method shall mean standpoint and not mere means.

A third and most marked characteristic of the genetic method is to be found in the claim made for it, that it offers a way of escape with regard to the question of *psychological causation*. As any student of psychological problems in any period of discussion readily discovers, the most vexing of these belongs to this very item, whether 'causation' shall be admitted in psychological theory; and, if so, how it shall be construed. Now it is affirmed, and then it is denied, that mental states can really be viewed as standing in a causal *nexus*. Witness the old-time debates about psychological motivation and freedom, and the but lately heated disputes over the scope and conditions of mind and brain parallelism. And that very question of our science itself: Is it right, proper, and within our power to have any knowledge of the mind which shall receive its support and scientific appeal solely in that type of explanation composed only of admittedly distinct mental factors? or, can we lay claim to a psychology only by deriving its foundations in the physical and chemical processes given immediately in the body, and diligently studied by all stimuli psychologists, on the one hand, and by appealing to a great so-called prototype of psychical changes which is given in the phenomena of life-genesis and is diligently studied by all biological psychologists, on the other? And one might go on to indicate other fundamental tendencies in psychology, all of which come back eventually to this query: What are the great terms of the mental life, and under what specific relation must they be handled before we can accredit the results of the science? This is, indeed, bringing into question, in an altogether negative and condemning way, the existence and the right to exist, of this science. And yet the psychologists fortuitously labor to increase our knowledge of mind, and there is apparent continuity in the progression of their specific and generic achievements!

I do not think this presents the situation too seriously, and the pivot on which turns the self-equilibration of psychology, as well as the acceptance of psychology by the self-constituted committee of award made up of those scientists already in the possession of the field of nature, is this matter of causation within consciousness.

The genetic method fancies itself seeing the impossibility of ever ending this debate on the grounds of its own assumptions, and rushes forward with a substitute conception. To this method, continual debate is conclusive evidence of error. The substitute eliminates the whole item of psychological causation, and, thus robbing the disput-

ants of their soaring inflatus, would let them fall to earth, dashing both themselves and their meaningless contentions to pieces. Indeed, the genetic method would do away with all 'causation' in psychology. This *nexus*, it is affirmed, is perfectly well known in the physical world, but is misleading and confusing when introduced into the mental world. This may essentially involve a monadism which is more or less distasteful to all psychology as such, but which is not disturbing to the method in its mission of philanthropy.

In observing this attempt to settle the old-time causation dispute in psychology by simply removing it, we come upon one of the fundamental characteristics which the genetic method displays, namely, its struggle to fill the void created by that removal. The method has no particular quarrel with causation as one of the working categories; it only struggles to put its own interpretation upon such a category. This it does by introducing the conception of 'stages' and their one relation, which is said to be 'genetic.' These stages offer an easy picture for the exercise of the scientific imagination. These stages are related only in time, and they stand in the order of succession. They are not toy stages, subject to the indiscriminate manipulations of the child playing with them; for this time order is made to do service in that great causal void. These 'stages' are 'lower' and 'higher,' and there is a necessary (*sic*) arrangement among them, both in nature and in the representing science. The 'higher' stage is later in time, and is conceived of as 'fully explained' by the next lower stage. Instead of cause and effect, the method pursues lower and higher stages, and instead of causal connection, it thinks only of stage transitions and successions.

To this characteristic dealing with simple 'stages,' there must be added the ramifications of the genetic conception in order to make it workable. There is psychological 'nascency,' psychological 'blooming,' and psychological 'decadence.' The aggregate of these is bunched into psychological 'recapitulation.' Individual stages may be 'furthered,' 'arrested,' 'appear' and 'disappear.' The effort is made to 'explain' the ages of the soul, rather than its fundamental or its incidental 'processes' as a given object in nature. Time is here made the great soul divider, forcing a cleavage which no efforts can ever be successful in reuniting. The standpoint of the method herein is practically an assumption, not of the uniformity of 'processes' in the individual mind, but rather of the uniformity of racial scintillations within the individual. In this way the individual conciousness, as known to or conceived of by the other forms of psychological thought,

is literally pushed aside, or 'transcended,' in order to get at the soul as a denizen of the world. Thus we see something of a logical sequence in the emphasis which the method places upon the *differentiæ* of eras and epochs in the individual, rather than upon the continuity of process in the individual consciousness.

The instruments known to psychological analysts, such as faculty, idea, conscious process, sensation, and so forth, are thus disposed of at almost one stroke. All those efforts of the genetic method are not to be identified or confused with 'psychological history,' which must always be limited to the individual mind. At the same time, an historical usage is made of the 'explanatory' coin of the method by having it pay for an archæological journey into the soul's domain, there viewing the slow and 'conscious' acquisitions which have preserved 'themselves'—but not their identity—in 'the neural and automatic and instinctive and unconscious processes' so readily traceable, as it is affirmed, in the geography of the soul of to-day. The 'over-soul' of a metaphysical psychology, even the common-place, observable soul of analytical psychology, is not known in this historical tracing. By such a long detour, the method of genesis endeavors to 'get into' the soul of the present day, by maintaining that other methods do not 'get into' the soul at all. Thus, psychology is only one of the racial means for aiding the soul to get inside of itself!

Are we to believe that the 'lower' stage, when ascertained, 'explains' the stage next 'higher,' when it becomes known? Are these stages static units, standing in the developing progression, which can be picked out and treated as units, even as the abstracted units of scientific thought? What but the 'higher' stage can determine what and which the 'lower' is? And, when it is demanded that our psychological quest shall be for 'stages,' then our science must frankly confess that it can never rise above the merely descriptive plane, and also that through the genius of genesis it has virtually fallen back into that estate which had been established unto it by the methods of speculative abstraction of a century ago.

This assumption—or perhaps, as truer to the facts, this unsecured loan from the biological scheme—that soul facts are self-explanatory when they are arranged in a 'genetic' order, remains wholly without an exhaustive examination. The logic of scientific judgments, however, cannot by any possible dialectic be identified with the morphological push to the genetic method on the grounds of mere science. Herein we have brought home to us a strong suspicion that the truth of the matter will probably be disclosed in the fact that the genetic

method is merely an exponent of a 'genetic' philosophy. Psychology is here under the constraining invitation to return to its earlier metaphysical imprisonment!

Suppose we have reason to hold, on the other hand, that real explanation—that something which alone can make our psychology a veritable science of mind—works in a direction directly opposite to that which evolution is said to pursue. Suppose it is held, in truth, that the higher 'stage' is first able to throw light upon, and to give meaning to, its predecessors. Such a view leaves abundant room for the genetic method to ply its profession constructively in a study of mind; but it does not dispose itself to allow the postulates of a method to displace the final necessity of specific intellectual activity being directed towards specific problems which confront one for 'explanation.'

Another defect in this doctrine of 'self-explanatory' stages is to be found in the violent contradiction between it and the practice of the geneticists. This practice takes its twofold foundation in the conception of *reflex action* (physiologically), and in the effort to trace all mental content from *one* specific, primordial *quality* (psychologically), such as mere sensation or mere feeling. The theory of this practice maintains that whatever the soul may be, it is naught but a variant of the original function, and that whatever the soul may do, is of the nature of a reflex, derived either from the proddings of a world environment, or from the elevation of the 'remains' of psychological ancestry—for it is not said there are no soul worms to destroy that ancestry—which are lugged into the precincts of the individual mind by the laws of soul heredity. Here we have a nest of incoherencies, which would not be permitted to prevail a moment in any 'natural' science other than psychology.

In the fourth place, we find that the genetic method is sometimes fondly regarded as the final and the highest method of psychology. This relative rank is put forward as one of its chief characteristics, and as one which therefore entitles it to the highest respect and to a throughgoing adoption by the science. *Ne plus ultra* is a fine thing to say of a method, and there are those among us who find in the tracery of beginnings the final outcome of one's thoughts about the soul. This practically says that, when in the spirit of this method, we traverse the country of the soul, we shall have exhausted its content and accordingly brought the science to its perfection. No more problems are then to remain. Idyllic contemplation will then sublimate all psychological work into æsthetic repose!

In reply to this great claim for the genetic method, it must be

emphatically stated that the pristine psychological method was the genetic, which was so remarkably applied by Aristotle, the founder of the science. To him, however, it was not the final goal of the science, but only a very convenient external scaffolding, which he rejected for his later penetrating analysis of reason. This mere historic precedence of the method may, however, leave entirely open the question of its validity for psychology; and its neglect from the time of its projector until the last century, to be sure, cannot be forced into an argument against its validity. The claim that it mounts above all other methods receives in this historic fact a severe shock, inasmuch as the definite progression of methods preceding its conception and application, demanded by the claim in order to be valid, is found to be lacking in the case of our great founder.

In the second place, it may be replied to this claim that, instead of bringing peace, it has brought the sword to psychology. Instead of introducing apparent unity and progress towards perfection in the science, the genetic method has thus far succeeded only in breaking up the subject and in scattering forces. Since 1855, the date of its extreme application, the method in its results has had a fitful history. This fitfulness can hardly be looked upon as a mere consequence of the number of 'new' problems which are possible from the genetic standpoint. Its adherents may, indeed, regard this condition as necessary and desirable before the method can rightfully come into its own. They may explain this turmoil as merely the inevitable progression from disparate judgments to the unity that is to come. This excuse for the effects of the method may be accepted for all that it is worth, while at the same time one must whisper a doubt whether the growth of a science is really advanced by revolution, which this method fosters.

The problem of genesis is a worthy problem, academic though it may at first appear to be, especially when the genesis is that of the human soul. The worthiness of the problem can be seen in its riper fruits in the psychology of the child, of the animal and of the race, which have grown under the inspiration of the genetic conception, but not as a result of the application of that method. These genetic branches of psychology have indeed flourished on the basis of the adaptation of particular methods of the science, to which that of introspection has always been ancillary. We must thus not fail to make the radical distinction between genesis as *one* of the veritable problems which we should raise concerning mind, and genesis as the *sole* problem of psychology, in the light of which the entire science is to be

reconstructed. The latter carries the identification of problem, method, and resultant science to an extreme. And often the last extremity of credulity is played upon in so doing, as may readily be seen if one attempts to catalogue some of the assumptions which support the method.

As a fifth group of characteristics of the genetic method, I herewith venture to set down, without accompanying discussion, some of the assumptions which envelope the standpoint of the method, giving it all the force to its arguments which they can possess. It is not meant to be implied that these assumptions are gratuitous and independent of the empirical necessities of a scientific adjustment of mental data.

The genetic method assumes that there is one or more definitive relations between mental states as they appear in the life of the individual mind, but not as defined by the limits of that one mind.

It assumes a 'racial' consciousness, which parallels that of the individual and, in fact, underlies that of the individual. From this unconscious background or underground to consciousness many of the features of genetic hypotheses are derived. While assuming the play of the environment of a psychological order as all-important in empirical explanation, this environment is sublimated by the heredity of soul, which forces one stage to succeed another irrespective of environment.

Prefacing the work of analysis as completely finished, it assumes 'feeling' in some one of its specific variants to be the primitive consciousness, both in the biological series of the cosmic order and in the morphological series which constructs the individual.

Along with the assumption of stages in the individual mind, which are residual of large eras of ancestral experiences, not necessarily human, goes the corollary assumption of psychological heredity and the inheritance of acquired characteristics, by means of which alone the alleged continuity of mind life, linking all individual souls into one unbroken series, is capable of breaking over the limits of the dead-level of heredity and providing for the manifest accretions to later forms of mental experience.

In going outside the individual's consciousness, *i. e.*, outside the individual organism, to get its explanatory basis and data, it adopts the extreme validity of analogy as the constructive method of science, in so far as it ventures to deal with the mind as a distinct object in the series of nature.

Such a catalogue of assumptions might be slightly increased, but

it seems to represent substantially just the status of psychological genesis when it is conceived of as the sole problem of psychology.

No discussion of the evolutionary reach of psychology would be at all adequate if it failed to treat of the probability which characterizes its final judgments. As we all know, the primary danger in psychology is that of accepting objective error, and not any subjective harm. And, indeed, the progress of any particular science can be marked better by the steps it has taken to eliminate its errors, and to increase the degree of probability which attaches to its conclusions, than by any other mode of tracing the activity within the science. In estimating the presence and scope of error, however, we must not fail to make the radical distinction between the mere fact of actually false statements, on the one hand, and the deviation of the statement of the individual from the majority conclusion of the group to which that individual may belong, on the other. The development of the experimental method and the earlier precise pruning of the introspective method have both alike been aware for the most part of this fundamental distinction.

All sources of error can be reduced to these: partial, false, or impossible observation; imperfect enumeration; and defect in argument in developing the inferences which shall be entertained. One might go even so far as to maintain that error is to be found solely in the absolute inadequacy and inappropriateness of method itself—as has actually been the case with those positivists who find psychological methods of any and every sort inherent perversions of truth.

The genetic method does not readily fall within any one of these groups of sources of error liability. Its most conspicuous relation to error liability is to be found in the fact that it introduces a new source thereof by increasing variations which are said to demand explanation, and in thereby multiplying probabilities. Tracing analogies and making minute comparisons between remote facts within the realm of nature spread the range of error enormously. It is not easy, however, to put one's finger upon the dependency of error upon this method, since its scope and practice lack so much uniformity. In its widest genius, however, as an effort to consistently regard the soul as a rehabilitation of the psychological foundations of its ancestry, we find its liability to error to be due to the extreme infinity of variations in that ancestry and in the almost impossible task of picking out the fundamentals, for whose variations in the individual the champion of the method is to search. The genetic method has no specific right to affirm that the ancestry of your soul and the ancestry of my soul have

identical lineaments which are open to microscopic examination by the method. The aggregation of rudiments in a modern soul cannot—at least on the basis of our present knowledge of the biological relations which hedge in the genesis of a soul—be said to follow any 'laws, without introducing greater liability to error.

Another source of error in the application of the genetic method is to be found in its characteristic intention to be more interested in a constructed order of facts than in the validity of the judgment which may be pronounced in the effort to 'explain' these facts. Behind this cultivated interest lies the profound, and as yet wholly unexplored assumption that these facts become self-explanatory when arranged in the genetic order, as already pointed out.

Another tendency to error in the adoption of the method is due to the characteristic fact that the method may be regarded as wholly artificial. Why the necessity for a *re*arrangement of soul-facts in the order which is not displayed by a descriptive history of the individual soul, is not made plain by any ordinary and readily acceptable postulate upon which the whole science may rest. Here we have ample reason to believe that the 'natural' order is decidedly not the 'explanatory' order. This statement, of course, throws us back upon the recurring question, Which of the two orders is prior? It practically amounts to a confession that consciousness is so poor in its resources that it is unable to tell its own story! And of all sad things of psychological tongue or pen, these words expressing despair of method are the saddest.

In close connection with the foregoing, one should not forget to mention both the possibility and actuality of the *misuse* of the genetic method. Its partial fitness for dealing with certain data disposes some users of it to insist that it alone is fit to handle all data. This misuse may be found among those who regard the ideal of method as a necessity for offering all sorts of fanciful and absurd constructions upon the basis of development. Those who fancy that clearness of idea and continuity of thought are just as essential in genetic science as in analytical science, are among those who first turn the suggestiveness of the method into a principle of logical dissoluteness. On the other hand, a method which is constantly subject to being misunderstood is one which should receive the most careful attention and criticism from those who insist upon regarding it as the final psychological judge.

In concluding this recognition of a method which can be made to stand out so completely as a distinct form of approach to the prob-

lems of the soul, and in recognizing a method which has well-nigh swept the psychological field from one end to the other, it would be ungrateful, to say the least, not to record also some of the good features which cling to this method. All psychological facts are given in time, and genesis depends upon this condition. The method enlarges the scope of psychology, admitting many more minds, and thus many more facts along particular problems. It is a distinct gain to be instructed that genetic facts abound in the animal series. Every state of consciousness has a 'morphological' history. The genetic method stoutly opposes—superficially at least—the principle of the conservation and correlation of energy (and one might smile happily when there is found some point which forbids the physicist entering the field of psychology—as physicist, that is). The method is constructive, and brings the individual mind into relations, derived from terms peculiar to itself, with other minds, as the race mind, and especially the mind in animals. Here are vast fields brought into manageable compass by this one method of world-comparison.

In attempting its vast generalizations about the soul the method does not outstrip the corresponding rights inherent in any of the opposite methods of the science. Undoubtedly it is worth something to our science if we can definitely relate the soul to the cosmic order; but we should not wink at the fact that even the more limited and empirical character of tellurian psychologizing always keeps us on *the outside* of the soul. Taken in its very best intentions, the method enlarges the scope of, and the need for, activity within the science by raising an indefinite number of new problems, or rather by bringing every department of psychological questioning under the necessity of working over its material again in the interests of genesis. Any point of view which can do this is surely of peculiar inherent value to science. The focus of the genetic method attempts to be human consciousness. The method attempts to mediate between the extremes of the deductive and the inductive methods. We all know how difficult real induction, as resting upon experience alone, is; and as well the inadequacy of deduction, which always makes use of the opportunity of constructing its own metaphysical assumptions to suit itself. This mediation attempts to bring the soul under the larger conception of 'life,' which is alleged to be the highest, and hence, the largest, concept capable for the scientific mind. Its bondage to biology and to the postulates of biology is herein depicted in a rather deplorable fashion. One of the very excellent features of the genetic method is its generous attempt

to bring about an equivalence of soul facts for the aims of science. The gesture of the Indian, the prying of the monkey, the 'scent' of the dog, the movements of the amœba, are ranked with the analysis of the introspectionist. But the method fails us in telling definitely where 'soul' facts begin, and where they end. It nowhere really develops psychological criteria, other than the vague generality of 'life' processes.

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PSYCHOLOGICAL LITERATURE.

THE SIGNIFICANCE OF MIND IN EVOLUTION.[1]

1. *Is Evolution Progressive?*—Most psychologists to-day believe in the evolution of mind, but the character of that evolution in detail and the stages through which consciousness has passed in the building up of mental structure are subjects which are still under debate. Hence, any attempt such as this of Mr. Hobhouse to add to the slowly-accumulating mass of reliable facts concerning animal consciousness and to interpret those facts in the light of the evolutionary hypothesis is worthy of careful study by the comparative psychologist.

The question concerning evolution, Is evolution continuous? which has agitated reflective thought for twenty-five years, has gradually been giving place to the question, Is evolution progressive? It is beginning to be recognized that, just as the continuity of evolution is not a dead mechanical uniformity but a vital reconstruction with what on the surface appear as gaps and leaps and zigzags, so that same evolution is not an unbroken upward course but a spiral movement of organization of structure and function in which the process ever and anon returns upon itself in an apparently retrogressive movement. A purely progressive evolution is a philosophic myth. All anagenesis must be read in the light of catagenesis. There is nothing in a mechanical conception of evolution to ensure that it shall be progressive.

What is the source of the ideas of progress connected with the conception of evolution? Do they arise from the inevitable tendency to read evolution backwards, to interpret the process in terms of the results, just as the mechanical and materialistic conception of evolution arose from interpreting the same process in terms of its inchoate beginnings? Is either interpretation a justifiable one taken by itself? The materialistic or mechanical evolutionist has been silenced. How about the idealistic or teleological evolutionist?

We cling with the greatest tenacity to the idea of progress. What could evolution mean for us, we ask, if it were not progressive! Yet consider the facts. "If the struggle for existence has produced the wisdom of man, it has also sharpened the tiger's claw and poisoned

[1] A critical review of 'Mind in Evolution,' by L. T. Hobhouse. London, Macmillan & Co., 1901. Pp. 406.

the cobra's fang" (p. 1). "In some parts of Africa the horse is driven out by the Tsetse fly; in others the white man succumbs before the malaria germ, just as in this country the honest investor goes down before the swindling company promoter" (p. 2). "The tendency of evolution as a whole is not to produce the highest type, but rather to produce as many types as possible" (p. 4). Can we believe in progression in the light of such facts? If so, in what sense are we using the word 'progress' and what factors in the evolutionary process are its guarantee?

2. *The Place of Reason in Evolution.*—This is the problem with which the first chapter opens. This chapter is entitled 'Mind as a Factor in Evolution.' The rest of the volume may be regarded as an elaborate attempt, in answer to the implied question, to show the significance of mind in evolution, and the dominant note in the answer is that the grounds for our belief in progressive evolution are to be found in the peculiar place which reason occupies as the consummation of the whole, becoming itself the guide in self-conscious development.

The generic function of mind in organic life and evolution is "the adjustment of action to the ends of the individual or of the species, based upon a correlation of past experience, present circumstances, and future possibilities" (p. 10). The vital essence of mind is the "bringing things together so that they have a bearing upon one another. Where there is mind there is order and system, correlation and proportion, a harmonizing of forces, and an interconnection of parts. The organism which is gifted with intelligence shows it by arranging its action on a certain plan. It adapts means to ends" (p. 6). Organized action is intelligent action. Unorganized action is unintelligent. Where there is no mind at work action is random, blind, isolated, conflicting. What is done is done on the impulse of the moment and not as a means to an end. Where minds differ is in organizing power —in scope or comprehensiveness of plan, and in method of construction or execution of the plan.

"The whole process of orthogenic evolution consists in the gradual replacement of instinct by reason, and it is the final goal of reason to do precisely what is ascribed to instinct—to bring all the experience of the race to bear in organizing the whole life of the race" (but in the case of reason to do this consciously, instead of unconsciously as in the case of instinct, p. 9).

3. *General Scope of the Work.*—Before speaking of certain special topics more in detail it will be well to take a bird's-eye view of the

whole book. 'Organic Adaptability,' 'Reflex Action,' and 'Instinct' are examined in turn (Chapters II.–IV.), with the result of finding a gradual increase in complexity and variability in the method of solution of the various problems of organic adjustment. As we pass upward in the animal scale we find that pure instinct gradually gives place to a modified and generalized form which, perhaps, is best called impulse. Out of this transformed instinct, by a process of 'Assimilation and Readjustment' (Chapter V.), is developed a stage of intelligence which is the first remove from instinct. This Mr. Hobhouse calls 'Concrete Experience.' It yields what he calls the 'Practical Judgment' (Chapter VI.). From his own experimental results (cats, dogs, monkeys, an elephant, and an otter) and from his study of the experiments of other investigators, the author is led (Chapters VII.–IX.) to the conclusion that animals share with man in the possession of 'Concrete Experience and the Practical Judgment.' The principal reason for the human ascendency is found (Chapters X.–XIV.) to lie in the possession of language and social organization, which serve to facilitate the development of 'Articulate Ideas' (Chapter X.) and thus to make possible those 'Products of Conceptual Thought' (Chapter XIII.) which are peculiar to man. Finally, in three instructive chapters (XV.–XVII.) the main steps in the evolution of intelligence are schematized (Chapter XV. on 'The Stages of Correlation') and the place of 'Self-conscious Development' (Chapter XVII.) is shown in relation to natural selection. A few of the more important points will now be discussed in more detail.

4. *What is an Organism?*—According to Mr. Hobhouse, "By an organic whole is understood one which (1) has a certain general character or individuality, while (2) it consists of distinguishable parts each with a certain character of its own, but (3) such that they cannot exist unmodified apart from the whole, while the character of the whole is similarly dependent upon them. * * * It may indeed be doubted whether a purely mechanical whole exists in *rerum natura*" (p. 374). The function of the organism 'may be to evolve carbonic acid, or it may be to produce poetry' (p. 12).

An organism is only a complicated machine. "Put a penny on a balance, and it weighs down the scale by the simple action of the lever. Put it into the slot of an automatic machine, and it produces a stick of chocolate by I know not what complication of levers and cogs. Complexity of adjustment does not take us out of the region of machinery" (p. 31).

In spite of such statements, the author still seems to feel that

there is some degradation in the idea of the organism being even a complicated machine, and goes on to show what he conceives to be the difference between an organism and a machine. The chief difference he finds in the power of recuperation and self-reproduction of the organism. This he seems to regard as constituting a qualitative difference between the two.

In reply to this we would raise the question as to whether the difference between the organism and the machine in respect to this matter of self-reproduction is not simply a difference in degree. It is clear that "the continued motion of the pendulum depends upon an outside force. It has no power of accumulating afresh the energy which it loses at each swing" (p. 19). But the organism is self-maintaining only within certain limits, *e. g.*, between birth and death. It is true that, "within tolerably wide limits, it can survive accidents which cause a considerable departure from its normal course, and either struggle back to the typical life of the species again, or effect some compromise with circumstances by which life is maintained in some more or less modified form" (p. 19). But it is only a prolonged life and a relative equilibrium, since both yield ultimately (in old age and death) to 'outside' forces. No organism, unless it be, perchance, the totality of the universe itself, is really a self-maintaining and self-sustaining whole. Mr. Hobhouse's own illustrations of the compensating pendulum and the linotype might be used to prove just the opposite thesis.

The same general remarks apply to the other characteristic of the organism which is taken as differentiating it from the machine—its circular or spiral process of waste and repair. Illustrations in the organism are respiration, the vasomotor mechanism for maintaining an even bodily temperature, and the circuit of hunger, eating, strength, exercise, hunger, etc. But "a machine can be made to regulate its own action within certain limits. Thus, in the steam-engine, the forward thrust of the piston opens and shuts valves by which the backward thrust is at once brought about. Here there is perhaps a parallel to the automatic rhythm of breathing or of the heart's beat. * * * By the device of the 'governor' a steam-engine can regulate its own available energy in accordance with the work required of it. There is a close analogy here to the labored breathing of hard exercise where more oxygen is required" (p. 15).

Nothing could be better than the author's statement as to the nature of the growth process which, it seems to the present writer, is much more in harmony with the criticism just made than with the

point of view defended in the text. There is "a certain equilibrium point, as we may call it, which the organism is always striving to maintain." "The equilibrium is a moving equilibrium." "The process of rapid growth in youth, the slow change during maturity, and the gradual decay ending in death * * * is the normal orbit of every organism. The equilibrium point moves along this orbit, and the momentary changes of growth are so many oscillations about the equilibrium point as it moves." "Every deviation from the equilibrium point," within certain limits, "sets up a tendency to return to it" (p. 13).

5. *Instinct and Impulse.*—Mr. Hobhouse says (p. 25) that he follows Preyer and Verworn in defining impulsive actions as actions which "are caused without previous peripheral excitement exclusively by the nutritive and other organic processes that go on in the motor centers of the lowest rank." "An impulsive movement is due to purely internal changes." Evidently he does not identify impulse with what he elsewhere calls 'internal disposition' (Stimmung, craving, p. 57; cf. 69, 70, 95), yet this is just what such writers as Marshall, Baldwin and Dewey seem to mean by impulse. According to Mr. Marshall an impulse results from the inhibition of an instinct. Professor Baldwin calls an impulse a 'snubbed instinct.' Professor Dewey calls an impulse an 'unravelled instinct.' The suggestion is that impulse represents the disintegration of instinct and carries with it a certain amount of consciousness. On this view, this 'internal disposition' of Mr. Hobhouse would be brought out only when the instinct is thwarted or interfered with in some way. Certainly, in all that he says on the subject of ethical impulse (pp. 314, 337, 354–355) and in much of what he says under the head of the method of 'perceptual learning' (Chapter VIII.) he leans toward this view of impulse.

This, however, is simply a matter of terminology apart from certain other considerations which are involved. The suggestion here made is that much light would be thrown on these other issues by a recognition of the true relation of impulse to instinct. This brings up the whole question of the emergence of consciousness within the life of purely instinctive action.

6. *Impulse and Consciousness.*—The most important of these other questions which are here involved is connected with the relation of instinct and impulse to consciousness. Mr. Hobhouse says distinctly that he will not raise this question (pp. 79–80) and then goes on to say that 'hereditary structure' supplies sensations to consciousness,

that 'hereditary responses' yield 'sensation and feeling' to man and therefore probably to animals as well. But the problem is, under just what conditions do 'hereditary structure' and 'hereditary responses' yield consciousness? Here is just where a true psychology of impulse fills a gap in the evolution of mind. As has just been said, Mr. Hobhouse does not elaborate this point, but his statement as far as it goes is almost identical with that of Mr. Morgan on the same point. It will be profitable to refer to the statement of the latter in this connection. The criticism offered upon his view will apply to the less explicit doctrine of Mr. Hobhouse.

Mr. Morgan ('Habit and Instinct,' p. 136; cf. summary, pp. 323 f.) says that "on the occasion of the first performance of an instinctive activity the coördination involved is automatic, and cannot be regarded as under the guidance of consciousness; but that the carrying out of the activity furnishes data to consciousness in the light of which the subsequent performance of a like activity may be perfected, or modified or checked." "From this it follows," he says, "that only on the occasion of its first performance does such a congenital activity present itself for our study in its instinctive purity. For on subsequent occasions it is more or less modified by the results of the experience acquired by the individual."[1]

Now the question is, How and why does an activity which is relatively so perfect as a congenital instinct 'furnish data to consciousness'? If the reaction takes place in the first instance without consciousness, why should consciousness be developed in connection with it later? What Mr. Morgan's theory lacks is a sufficient explanation for the emergence of consciousness at this point, or for the fact that the 'carrying out of the activity' at this point or at any other point 'furnishes data to consciousness.' But on the theory which is defended by the present writer,[2] the emergence of consciousness is connected with some break in the adjustment process by which the animal is endeavoring to adapt itself in its environment. The definite congenital instinct will continue 'in its instinctive purity' as long as the process of adaptation runs smoothly. But if there is any serious friction in that process of adaptation, some new reaction is demanded, or some modification of the old one, and it is at the point of and for the sake of this new need of the organism that consciousness appears as the medium in which the new mode of response is built up.

[1] Cf. also his statement in 'Animal Behaviour' (1900), p. 332, that 'organic evolution provides ready-grouped data to consciousness.'

[2] Cf. *Journal of Comparative Neurology*, Vol. XI., No. 2, for a fuller statement.

Mr. Marshall speaks of 'instinct-feelings' as the subjective accompaniments or conscious coincidents of 'instinct-actions' ('Instinct and Reason,' p. 86), and shows how this 'consciousness coincident with the instinct-actions' arises. He shows that impulses "are mental phases which in an objective view we always find to be determined by the inhibition of instinct-actions as these are more or less modified by experience; which instinct actions have been stimulated by the presence of conditions that might normally call them out, but which for one reason or another are not at once realized" (p. 94; cf. Chapter XIII.). He says (p. 342) that every instinct "implies the possibility of the appearance of an impulse, provided the conditions of stimulation appropriate to the expression of the instinct are realized, yet under certain forms which restrict this expression." He denies Professor James' contention that 'every instinct is an impulse.' "When we see a man aim a quick blow at an enemy suddenly appearing before him we say that the actions involved express the instinct or capacity within him; but it is when we see him restrain this action under temptation that we properly say that he must have had an impulse which would have led him to strike his enemy had it not been restrained in one way or another" (p. 342). "We act instinctively in a thousand different ways during all our life without paying any attention to the acts; but some day, when something inhibits our instinct actions, then we have a disturbance of our mental life, which in complex cases produces what we designate an impulse" (p. 343). "In no case does the impulse appear in consciousness except as the result of an obstruction to the realization of certain activities which are determined by the existence within us of coördinated neural structures" (p. 344). The activities, if they find expression, which accompany such a state of consciousness are random, uncontrolled, haphazard, unmediated. Impulse is characteristically accompanied by an emotional consciousness, by what Mr. Marshall calls 'instinct-feelings.'

This, as I understand it, is the essence of the view of impulse as held by Professor Dewey and Professor Baldwin. The original fact of all experience is its character as a movement or tendency to action. Interrupted or obstructed activity gives rise to feeling. Out of feeling sensation or cognition is gradually evolved. Impulse represents the transition, the emergence of the conscious out of or, better, within the unconscious. Mr. Marshall supplies what is lacking in Mr. Morgan and Mr. Hobhouse. The bearing of this on the theory of the criterion for the presence of consciousness (Hobhouse, p. 82) and of the distribution of intelligence in the animal world (pp. 103–111) is

obvious. Curiously, there is no mention of Professor Loeb's researches.

7. *Instinct and Reason.*—Intelligence Mr. Hobhouse defines as 'the power of an organism to adapt action to requirement without the guidance of a hereditary method of adjustment' (p. 82). In the growth of this power of correlation of its own past experiences with its subsequent action lies the evolution of animal mind. But instinct and intelligence or reason, though opposed in idea, are so far from being incompatible in fact, that it is actually within the sphere of instinct that intelligence first arises (cf. 270, and especially, pp. 77–79, a remarkably good passage). Here, again, by some strange oversight, there is no mention of Mr. Marshall's almost identical view in his 'Instinct and Reason.'

First, it is pointed out in what respects the scope of animal consciousness is restricted (pp. 312, 314, 315, 320, 321). Second, it is shown how in detail reason develops within instinct (pp. 58–79). "The impulse to reason is itself an instinct" (p. 318). Third, instinct furnishes the main outlines of experience, even in the case of man who possesses reason. "Instinct lays the ground plan of conduct, within which, details may be remodelled by individual experience" (p. 320). Instinct is finally transformed and evaluated by reason while still furnishing the content of experience (pp. 357, 370–372). Fourth, the elementary form of reason consists in connecting hereditary modes of reaction (instincts) with definite objects or situations—'a form of the defining or particularizing of instinct' (pp. 107, 108). Fifth, it may roughly be said that instinct (when broken up in impulse[1]) presents the ends of experience, and reason works out the means. "The first function of intelligence is to define the proximate ends of instinct, and thereby to render experience available in the choice or revision of means" (p. 270). Finally, the chief difference between man and the lower animals is that in the lower types of consciousness the attention is concentrated on the response rather than on the stimulus in the organic circuit (cf. pp. 142, 143). The civilization of man is simply the concentration of attention on the stimulus, on a large and elaborate scale. The response is the first phase of the organic circuit to come to consciousness because it is most directly connected with action. This might have some bearing on the difference between the so-called 'sensory' and 'motor' types of subjects in reaction experiments.

8. *Perception and Conception.*—The use of the term 'perception' by the author is open to criticism. It is difficult to believe that

[1] As above outlined.

there is any ambiguity in Mr. Hobhouse's mind as to the relation of perception to conception, but certainly it is not made in this book to stand out as clearly as his use of the term 'simple apprehension' in his valuable 'Theory of Knowledge.' 'Perceptual acquisition' (learning by perception of results) is distinguished from 'motor acquisition' (learning an act by doing it), on the one hand, while it is quite carefully marked off from conceptual processes, on the other. The use of the term is a common enough one, but not the best one, for perception arises only *with* conception, whereas Mr. Hobhouse, like Romanes and others, makes it precede conception.

His use of such terms as assimilation and association and practical judgment are apparently an attempt, like the term reception of Romanes, to fill the gap between perception and conception when thus conceived as successive stages of cognition instead of as complementary aspects. One sees evidence of the influence of the English empirical psychology in certain passages (cf. pp. 135, note, and 135–136, summary). Mr. Hobhouse says that 'assimilation does not necessarily involve ideas at all' (pp. 113, 114). How he is to reconcile this with his other statement on the previous page (p. 112) that all experience 'is in a sense experience of a relation,' is not made clear.

According to Mr. Hobhouse, where there is analysis of the relations implicit in a perception we get conception. But the knowledge of the situation as a whole without the relations being dissected out as distinct elements is perception (p. 117; cf. p. 124 and summary, pp. 135, 136). It certainly is an unfortunate use of the term 'perception,' because of the ambiguity. What we need is some unambiguous term to mark off the indeterminate stage of cognition from that determinate stage in which both the objects (of perception) and the relations (of conception) are distinguished within the cognized whole.

Mr. Hobhouse quotes Professor Thorndike with approval when he says that the animal, like the man swimming, 'simply feels an impulse from the sense-impression,' and implies, at least, that the animal has the *sense-impression* of the water, the sky, the birds above, as well as the impulse. But is not just this distinction the mark of the higher type of intelligence? The animal, like the man swimming or playing tennis, does not feel the sense-impression as such at all. He feels the impulse and it is only a subsequent analysis which reveals that the sense-impression was latent in this vague kinæsthetic consciousness of impulse. Perception and conception represent not successive stages but correlate phases within the cognitive function: they appear together and they vanish together.

9. *Methods of Learning.*—One of the most helpful discussions in the book is that which attempts to answer the question, How do animals learn? According to Mr. Hobhouse, the fundamental underlying principle of learning is that a wave of excitement once started in the nervous system persists for a certain short time (p. 93). Out of this grows the possibility of 'assimilation and readjustment.' This elementary correlation is the first stage beyond instinct in the evolution of mind. Its operation is confined to adjusting reactions suitably to their immediate results. If more remote correlations are to be effected, it must be by a very slow and gradual process. This renders it possible for animals to thrive without highly definite instincts and brings about the substitution of more or less general tendencies and impulses for the more narrowly defined hereditary methods of action.

It is not necessary to dwell on the unlearned type of reactions or the instinctive method of adjustment, except to say that in this connection the author calls attention to many human adjustments which are still practically on the animal level (pp. 310, 311, 318). More important is the method of trial and error or, as Professor Thorndike calls it, the method of trial and success. Mr. Hobhouse calls this the method of assimilation. All learning by experience has in it the element of rationality. Pleasure and pain play an important part in 'stamping in' a response (pp. 85 f., 98, 99, 141, 142). One trial may be enough to teach the animal, but the effect of the experience tends to wear off with time (p. 87; cf. 84). Most interesting is the treatment of what Mr. Hobhouse calls 'the critical success' (pp. 204, 267, and *passim*). Mr. Small, in his study of the rat, has called attention to the same thing. In the function of the critical success we have the first intimation of the method of learning by ideas. The real transition from the brute to the human took place when the animal first became conscious of the fact that intelligence or ideas are a valuable factor in the struggle for life as well as mere physical prowess or brute force. It is immaterial whether this discovery was made first by a biped such as the anthropoid ape or not. Whenever it was made, it placed the animal at an immense advantage over its fellows. Henceforth rational acts are not wholly of the trial and error type but involve the beginnings at least of deliberation in its twofold aspect of reflection and anticipation. Rational acts from this point on become more or less spontaneous and free instead of the result simply of necessity and coercion. "As ideas become more articulate, the results of experience are more freely combined or modified to suit practical

needs. Something like originality begins to show itself, and we have instances of what we have called 'spontaneous application'" (p. 234).

The three modes or types of reaction may be illustrated as follows. "Three persons start for a certain place. One does not know the way, but is directed to follow a certain road. Keeping to this road, he arrives safely and speedily unless there should be any unforeseen obstacle, such as a broken bridge, in which case, as he knows no other paths, he is blocked" (this is the case with the instinctive method of reaction). Another "wanders at random, but as everywhere there are hedges and walls preventing him from getting far out of the way, and as hedges grow up behind him to prevent his return, he gradually arrives by eliminating all possibilities of going anywhere else" (this is the method of trial and error). The third "knows where the point is, and finds his way there, going by a detour if the direct road is impassable" (this is the method of ideas, p. 400).

10. *The Function of the Universal in the Evolution of Intelligence.*—"We are thus brought to the primitive function that ideas fulfil in conduct. As long as impulses are fixed in relation to stimulus, whether by heredity or habit, action neither requires nor tolerates any further guide. But if the ends of an impulse are to be served by actions varying from case to case, a uniform reaction to uniform stimulus will no longer do, and the case is met in the human world by a formulation of the end to which one is impelled, along with its relation to the surrounding circumstances. The formulation of an end constitutes an idea, and the impulse so qualified becomes a desire. In other words, so long as stimulus guides action in a uniform manner, no idea of the end is required. Where the point to which action is directed must be defined specially for each action, there an idea is, in human conduct, necessary" (pp. 130, 131). This may be regarded as the particularizing function of the idea.

"The universal judgment is not so much a reference to an indefinite number of particulars as a rule of reference. * * * Its function in thought is (*a*) to sum up the result of a mass of experience, and (*b*) thereby to form a guide in dealing with a further mass of experience to come. Under both aspects it brings the action of the moment into explicit relation not merely with the immediate circumstances of the particular end, but with masses of experience past and future" (pp. 298, 299). The universal thus renders explicit 'influences which have already been operative without being expressly formulated' (*e. g.*, instincts, habits). 'When the results of experience can be rapidly

summed up and communicated' in and through the universal, action becomes more effective. This ability to build up a world of ideas or of universals by a correlation of his experiences in masses or systems is one mark of man's ascendency over the lower animals (p. 299). This may be regarded as the universalizing or generalizing function of the idea.

In the pre-reflective stage "what corresponds to the major premiss is a certain formed disposition, what corresponds to the minor a stimulus, what corresponds to the conclusion a response" (p. 323). "The premisses in this case are antecedent conditions from which the response follows, but there is no evidence that either of them is grasped in relation to the response or its results" (p. 323). In the stage of the practical thinking of the plain man and of the higher animals "the starting-point is a perceived relation — as of action and consequence — an 'observed particular,' and the result a judgment equivalent to the combination of minor premiss and conclusion, the major being still represented only by the mental habit which predisposes toward the combination" (p. 323). "In the reflective stage the major premiss itself becomes explicit and the syllogism complete. We have a universal judgment, the particular, and their combination in the conclusion. * * * The apprehension of the universal appears as a turning round of the mind upon its previous operations; a bringing into clear consciousness of what it was doing before" (p. 323). "Thought in this stage may therefore be typified in the completed syllogism with explicit major premiss as contrasted with the truncated syllogisms of the previous stages" (p. 325). Nowhere have I seen a statement which brings into clearer relief the true relation between the *idea* of psychology and the *universal* or *concept* of logic, and the relation of both to the pre-conscious modes of action. In this connection attention should be called to the valuable discussion in Chapter VI. of 'Concrete Experience and the Practical Judgment.' The practical judgment, according to Mr. Hobhouse, is intermediate between habituation on the one side and reasoning on the other, and in it he finds the key to the behavior of the higher animals as well as to many acts of human beings which are still on the animal level. Chapter XV., which is a 'Summary on the Stages of Correlation,' ought to be read in connection with the article by Professor Dewey on the same subject in the *Philosophical Review* ('Some Stages of Logical Thought,' September, 1900).

11. *Natural Selection by Conscious Adaptation.*—We may now return to the question with which we set out, Is evolution progressive? and answer that evolution along the line of rational intelligence

is progressive, because its guarantee lies within itself. All other evolution either tends to be stationary or tends toward retrogression. "The truth is that organization as a method of maintaining the species is set from the first in antithesis to natural selection. Natural selection rests on destruction. It maintains the type only by sacrificing the majority of individuals" (p. 387). "Natural selection can preserve and augment nothing that is not immediately useful. If the pigment-fleck which is the first rudimentary germ of the eye is preserved and developed, it must be because it is useful as a pigment-fleck. The plea that it will later develop into a magnificent sense-organ of the highest possible utility could not avail it in the court of natural selection unless it could prove services actually rendered by itself" (p. 391).

But with intelligence comes a latitude and development in scope as well as refinement of organization, and this latitude is a necessary condition of the highest development. The advent of intelligence means a revolution both in the method and in the rate of progress in evolution (cf. pp. 382, 383, 401–403). "Organization, especially in the form of intelligence, sets rather to maintain the individuals, and in so doing improves the type. The rational organization of life, from the dawn of parental care upwards, tends to suspend the struggle upon which natural selection rests * * * culminating in the deliberate self-development of a race under the guidance of reason. Organized life rests not on internecine rivalry, but on mutual interdependence" (p. 388).

Natural selection means limited supply of food, overproduction of individuals, struggle to the death of these individuals for this food, and either the elimination or the involuntary mutual adjustment of the individuals. Self-conscious evolution means controlling the supply of food and the environment in general, controlling the production and education of individuals, and the conscious coöperation of the individuals in mutual adjustment. This is just as *natural* a process of evolution as so-called 'natural' selection. The anti-social unethical individual goes to the wall here just as the weakling goes to the wall in the struggle for life. But the 'struggle' here is to make the unethical individual ethical, to make him worthy to survive. The category of worth and right transforms the category of chance and might, and the survival of the fittest is changed from the heartless competition of the ruthless struggle for life into an ethical coöperation of those who have survived to the end of fitting as many as possible to become worthy to survive. Pains and pleasures are substituted for death and life as the sanctions of conduct (p. 388), and, under the guidance of intelligence,

it is possible and even probable that "among the pleasures on which experience lights should be some connected not with the maintenance of the race at its then level, but with the further expansion of its powers. Such an expansion may be of little use to it as a means of survival for the present, but it means progress hereafter. Something like this would seem to be the history of those mathematical and æsthetic 'faculties' which have been a stumbling-block to natural selection" (p. 390).

But natural selection and conscious adaptation are not two antagonistic or incompatible processes. There is evidence that these coöperate in many instances for the preservation of a structure and its function. Thus, to use Mr. Hobhouse's own illustration, the contact of the hairs or the odor of the meat leads the insect to deposit its eggs, and this is a conscious reaction. But these insects attach themselves with equal readiness to any other hairy surface (besides the body of the bee) or deposit their eggs in the flowers of the carrion plant, the smell of which resembles that of putrid meat, and in this way many perish. The so-called law of chance and the principle of natural selection obviously enter here. Thus the actual survival of the type depends upon the concurrent operation of blind natural selection *and* a simple mode of conscious adaptation (cf. pp. 49 and 52).

Just because consciousness follows the center or area of tension or effort in the adaptation, the great mass of intelligent reactions has become automatic: these are preserved as unconscious reactions by heredity. "The smell of putrid meat attracts the gravid carrion fly. That is, it sets up motions of the wings which bring the fly to it, and the fly having arrived, the smell and the contact combined stimulate the functions of oviposition. The sight of appropriate food stimulates the chick to peck, just as the contact of the food with the interior of the bill stimulates the swallowing reflexes. And just as the sight of the food stimulates the chick to peck, so the sight of the chick stimulates the hen to cluck, or to scratch for food, or to protect it from danger, and so forth" (p. 53).

Now this may take place all unconsciously, automatically, or its salient feature may be conscious and voluntary. It may be all a mere matter of 'response of inherited structure to stimulus,' and again it may not. That depends upon circumstances. The whole chain of reflexes involved in any such instinctive act would not, of course, be in consciousness. The greater part of any organic circuit has been mechanized and relegated to the unconscious background, the subliminal life. But the salient feature, the prominent or crucial aspect,

will remain in the focus of attention or, if partially mechanized, will be brought back into the focus of consciousness because of the organic tension at that point. If the animal encounters no difficulty in finding the proper place to deposit its eggs or to find its food, these processes might go on in a purely reflex way. But if there is opposition to be encountered and if a search is necessary and if there is danger to be avoided, then it is most natural to suppose that there is a corresponding consciousness which serves both as a monitor and as a guide, not taking the place of instinct or reflex action, but directing it to finer issues or adapting it to varying circumstances. As Mr. Hobhouse himself remarks, it does not follow because an animal performs a given series of acts mechanically on one occasion, that that animal is destitute of consciousness, or that it is not able under *any* circumstances to bring consciousness to bear on the action. Under other and perhaps greater conditions of tension the situation might call forth a conscious reaction. "Many human actions are performed mechanically day by day, but a sufficiently strong stimulus directs attention to them, and brings intelligence to bear" (p. 55).

12. *The Principle of Projected Efficiency.*—Mr. Hobhouse's general conclusion is so similar both in spirit and in mode of statement to that of Mr. Kidd in his recent book on 'Principles of Western Civilization' (Macmillan, N. Y., 1902, pp. 538) that a brief comparison of the two may be instructive. Mr. Kidd writes, "The winning peoples who now inherit the world are they whose history in the past has been the theater of the operation of principles the meaning of which must have at every point transcended the meaning of the interests of those who at any time comprised the existing members of society" (p. 5). "The controlling center of the evolutionary process in our social history is, in short, not in the present at all, but in the future" (p. 6). This Mr. Kidd calls the 'principle of projected efficiency.' It follows that "the determining and controlling end towards which natural selection has been operating, must have been, not simply the benefit of the individual, nor even of his contemporaries, in a mere struggle for existence in the present, but a larger advantage, probably always far in the future, to which the individual and the present alike are subordinated" (p. 50).

Compare with this the words of Mr. Hobhouse. "We can conceive as not indefinitely remote a stage of knowledge in which the human species should come to understand its own development, its history, conditions, and possibilities, and on the basis of such an understanding should direct its own future" (p. 336). "Remote as this

ideal organization of life may be, it is suggested that the trend of theoretical science is towards the discovery of the conditions of human development, while the trend of the ethical spirit is towards making development the supreme object of action. In the union of these movements, human thought would seem to come as near as possible to the limiting conception of the correlation of all experience with all action. At any rate, knowledge of the underlying conditions of development would become the basis of a system of conduct designed to promote development. The life of the species would become self-conscious, and its growth self-determined" (p. 357). "We start with a consciousness limited to the reaction of the moment, and knowing nothing of the past which determines its action, nor of the future which its action will affect. Step by step, as we advance, more of the past and the future come within the scope of intelligence, and we end at a point where all that has made the race what it is is brought into the account and made to prove what it has in it to be. At this stage the mind of man is first fully self-conscious in the strict sense—conscious of its own nature, of the conditions under which it lives and works, of the future to which it may aspire" (pp. 336, 337).

It is stimulating to find in the sequel that the evolutionary theory has really been taken seriously and not, as is the case in so many books which profess to build on the doctrine of evolution, as a mere architectural convenience in the construction of an absolute system on a fixed preconceived foundation. The following quotation may fitly close this review, for it sums up what, in the present writer's opinion, is an insight into the true philosophy of evolution:

"Now it is easy to show that in such a system the ultimate ground of interconnection can neither be purely mechanical nor purely teleological. Reality is or includes a time process. Now, if we take any time process, and consider its beginning, we are dealing with a partial fact, and for every partial fact, thought demands an explanation which will connect it with reality as a whole. For the cause of the origin of a process, then, we may look in two directions, to its results or to its antecedents. If we look to the latter, we are clearly going outside the process. But if the process is one in which the whole nature of our ultimate system is to be expressed, we cannot go outside it without denying the claim of our system to be complete. We are therefore thrown forwards towards the results of this system. But neither can the purpose achieved by the process stand alone, for the necessity of the process must also be made plain. If an unconditional purpose were the secret of the universe, there could be no

explanation of the means, the process, and the effort through which the purpose is realized. From the conception of purpose, then, we are again thrown back on origins, just as these throw us forward to their purpose. We have, in short, to conceive a single principle not realized in full in any one phase, but pervading the whole world-process. In this principle, the possible and the actual in a sense come together, for what it is to be is an integral condition that goes to make the world what it is. We cannot take any phase of reality as an absolute starting-point and regard it as determining everything that follows upon it mechanically, or everything that precedes it teleologically. If we conceive any process as making up the life of an intelligible world-whole, we must conceive its origin and issue as dependent on and implying one another. That is, we must conceive it as determined organically" (pp. 404, 405).

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Psychology, Normal and Morbid. By CHARLES A. MERCIER. London, Swan Sonnenschein & Co.; New York, The Macmillan Co., 1901. Pp. 518.

There are few fields of psychology that require illumination more urgently than the relations between the normal and the abnormal; there are few topics in psychology concerning which a readable volume would receive a more cordial welcome than the topic which Dr. Mercier's title suggests. One of the phrases that the author lets fall, apropos of a very different application, is that 'the practice in psychology is that anyone may call anything by any name that he pleases.' The restrictions opposed to the indulgence of this propensity in psychology, are perhaps less stringent, yet not different in type, from those obtaining in other disciplines. Yet Dr. Mercier has used—I had almost said abused—this privilege in the correlation of content and title in the present volume.

It is always a delicate, sometimes an impertinent, task to find fault with an author for not doing that which he never intended to do. The 'pursuit of happiness' clause when applied to authorship seems to include the privilege of planning one's own tour, choosing one's own vehicle and route, taking one's own pace, and letting those follow who will. None the less from the point of view of serviceability, either as the record of an individual journey, or more directly as a guide-book to other tourists, the consideration of the probable interest, capacities, general information and equipment of the travelling public, will not

only in the long run, but in a comparatively short run, determine the longevity of any such contribution as Dr. Mercier has prepared.

It is rather easier to describe what the volume does not accomplish than what it does; the needs which it fails to meet, than the interests to which it will specially appeal. Lloyd Morgan's 'Introduction to Comparative Psychology' furnishes a possible model for a similar treatise upon abnormal psychology. The plan pursued is the presentation of those aspects of mental processes in general which find their most direct applications in the special field (comparative or abnormal psychology) concerned; this in turn followed by a consideration, in the light of principles thus established in normal psychology, of the special problems of the genetic or disordered mental processes. Such a volume has the possibility of serving as an introduction for students of normal psychology to the field of the abnormal, and of supplying those whose main interests are in the abnormal with a suitable foundation for their superstructure. Maudsley attempted such a service many years ago and (considering the psychological needs and interests of the day and his own purposes) with distinct success; the recent book of Dr. Störring aims to meet a similar need. No one, to my knowledge, has pursued so systematic a plan as Lloyd Morgan, or has performed so serviceable a function for abnormal psychology as he has done for comparative psychology. The hope of such service aroused by Dr. Mercier's title and by the ability shown in his previous contributions, is not justified. This field remains free for further effort.

Looking aside, however, from this purpose—a purpose, which Dr. Mercier's preface would indicate that he really contemplated—there are many things to be said in favor of the interest and value of what the volume contains. Not the least of these is that the book contains few dull pages; it is thoroughly readable. The style is clear, the illustrations apt, the central purpose in the several discussions well maintained, the positions stated with originality, and defended with ingenuity. It is a volume that the psychologist will assign to an accessible place upon his shelves.

The most striking feature of the group of topics represented is the prominence of the discussion of logical principles. Nearly half of the extensive volume treats of matter usually assigned to books on logic. Dr. Mercier does this with distinct intent, holding that the study of thinking is essential to an understanding of right and wrong, normal and morbid thinking. That part of the discussion that is distinctly aimed at the psychological analysis of belief-formation is,

indeed, pertinent; and the section upon delusive beliefs is one of the best in the volume. But in spite of a deep interest in the logical discussions, I cannot convince myself that more than one third of the discussion there introduced has direct pertinence to the main theme, nor that the general psychological reader will so far tolerate these discussions as to read them, or reading them, profit much by them. Students of logic will do well to consult some of Dr. Mercier's discussion; they, however, are not likely to look for them in a volume on abnormal psychology.

Apart from the consideration of the nature and validity of the thought-processes, both inductive and deductive, we find a presentation of the function of sensation, of memory, of volition, of pleasure and pain, and of consciousness, in the mental life; and in each case, though in a subsidiary and less extensive manner, some consideration of the abnormal aspects of these processes and their contribution to the symptoms of mental derangement. The interest and point of view throughout is that of the analytic student of normal mental states, utilizing the data of mental disease. No systematic account of the types or forms of occurrence of mental abnormalities will be found; in other words, the discussion is addressed to the professional student of normal psychology; the alienist seeking aid and enlightenment on matters psychological is not likely to be encouraged if his interests happen not to be those of Dr. Mercier.

In fine, the volume represents an able, suggestive, original, clear and helpful discussion of a group of main problems of the psychological life. The tone and temper and content of the discussion are the individual expression of the nature of Dr. Mercier's interests. It is a personal contribution and as such must in the end be valued. As a guide-book it seems destined to furnish readability combined with disappointment. As a record of what one traveller had eyes to see, and a ready pen to describe, it will have considerable value to others intending to travel, in this part or in that, the same route. This individual interest has led Dr. Mercier to adopt the methods of a pioneer in regions in which very considerable clearing of the ground had already been done. His utilization of the works of others is inadequate, leading him in lesser instances to profess originality in matters which others had previously set forth.

My chief contention is my chief regret—that Dr. Mercier did not decide to write a text more systematic, more consistent with his title, more clearly adapted to the services now most urgent in the field of abnormal psychology. As a series of chapters contributory to a clearer

analysis and portrayal of a group of psychological processes, the work promises to acquire decided value. It stands as Dr. Mercier's most important, as it is his most comprehensive, contribution to psychology.

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VISION.

A Method of Mapping Retinal Circulation by Projection. R. M. OGDEN. American Journal of Psychology, Vol. XII., pp. 281–291.

Mr. Ogden's method consisted in having his subjects fixate on a bright cross-lined screen, and observe the course of those fleeting bright points which are evidently connected in some way with the retinal circulation. The subjects carefully noted on similarly cross-lined paper the form and directions of these movements; and as a check and supplement, also observed and recorded the projection of the blood vessels produced by light playing upon the retina through a small moving aperture before the pupil. Two sample maps obtained in this way accompany the article, and from these it would appear that the courses of the moving points often exactly coincide with the shadow-projection on the blood vessels, thus adding to the evidence that the darting points are due to the flow of the blood. It also appears from the maps that the observation of the moving points gives courses that do not appear at all in the projection of the blood vessels themselves; although it is also true that in numerous instances blood vessels appear as shadows, of which there is no indication from the darting points. A farther advantage of these maps over those obtained solely by vascular projection is that they give the direction of the flow of blood in those vessels where the bright movements are noticeable.

The author believes that the evidence favors the view (modified from Helmholtz) that the more definite bright moving forms are due to 'chance spaces between corpuscles or bundles of corpuscles in the normal flux' of the blood. That they are not the projection of the moving blood corpuscles themselves seems probable from the fact that the spots are brighter even than a bright field. If there were to be any projection from them at all, it ought to be darker, since the shape of the corpuscles is not such as to concentrate the light, as Boisser held. The paper includes a good historical account of the method of observing retinal circulation, as well as of the theories to account for the darting points in the visual field. GEORGE M. STRATTON.

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Uber stereoskopische Lupen und Brillen. EMIL BERGER. Zeitschrift für Psychologie und Physiologie der Sinnesorgane, Vol. XXV., pp. 59–77.

Die normale Refraction des menschlichen Auges. M. STRAUB. Zeitschrift für Psychologie und Physiologie der Sinnesorgane, Vol. XXV., pp. 78–100.

These two articles are primarily for the oculist and optician, but are not without interest for psychologists as well. Dr. Berger describes an improvement he has made in the *loup*—the eye-glass used by watchmakers, engravers, and others employed in very fine work. In spite of the many advantages of binocular instruments, no binocular form of the loup has hitherto found favor, because the field of view in binocular instruments is much reduced and the magnifying power is usually too great. The author, starting from the form of glasses used by Brücke and by Liebreich (where short-focus convex lenses for each eye are placed so that their centers are closer together than the interocular distance) reports that he finds a great gain by simply inclining the glasses toward each other, not more than 15°. The nasal portion of each lens now inclines inward and backward, the temporal portion outward and forward. The result is a much larger field, all strain of accommodation and convergence, it is said, is done away with, and there is a heightened binocular depth-effect. Unevennesses of an observed surface amounting to $\frac{1}{50}$–$\frac{1}{100}$ of a millimeter are found to be noticeable.

Dr. Berger lays great weight on this horizontal inclination of decentered lenses. A certain amount of astigmatism results from this arrangement, but it is equal and opposite to the normal astigmatism of the eye; it is consequently no disadvantage, but rather a correction. The author has also devised a modified form of his loup which he calls stereoscopic 'spectacles'; these have an advantage for certain kinds of fine work. He even recommends in many cases the use of inclined glasses for ordinary reading, to avoid the disturbances of coordination of the two eyes which the usual glasses often produce. But for these details, well presented with diagrams, the original paper must be consulted.

For the psychologist, one of the most important points of the paper is the evidence brought forward that stereoscopic vision can improve or deteriorate according to practice. Many persons whose work is monocular—the Swiss watchmakers, for instance—find it of advantage to drill themselves, outside of working hours, in binocular vision. The neglect of the field of one eye while working with the

monocular loup tends to the neglect of that field even when the loup is not worn. The psychological suppression of the image in one eye often causes this eye to cease its suitable muscular coördination with its fellow; strabismus results, and the person becomes incurably monocular. The use of the binocular loup, Dr. Berger reports, has in his own case helped him to make good the loss he had suffered through the use of the monocular microscope. Several watchmakers reported good results also from the use of the ordinary stereoscope, as a corrective of the results of their prolonged monocular work.

Dr. Berger's use of the word 'stereoscopic' in connection with his own instrument should not mislead. He himself points out that its optical principle is entirely different from that of the stereoscope. His motive for introducing the word seems to be to give emphasis to the binocular depth with which his instrument affords, and even exaggerates, when one views solid objects through it.

The central thesis of Professor Straub's article is that the normal human eye, when accommodation is artificially annulled, does not focus distant objects on the retina, as has usually been held. Instead of being emmetropic, the normal eye is anatomically hypermetropic, and is made emmetropic only by the tonic tension of the ciliary muscles. The normal anatomical under-refraction, which is physiologically offset in this way, amounts to 1–1.5 dioptres.

This underlying anatomical hypermetropy appears even in a larger measure at birth, when it amounts on the average to about 2 dioptres, and with wider variations than in later life. After birth it gradually diminishes, to increase again with old age. Only the more decided hypermetropy, coming usually after 60 years of age, is to be attributed, with Donders, to a change in the refractive power of the lens. The milder forms of senile under-refraction the author believes (as against Donders) are due to the loss of ciliary muscular tone, and a consequent return to the normal anatomical refraction of the eye. The variations of normal refraction are thus in a large measure physiological; only at the beginning and end of life is there much of an alteration of the passive shape of the eye.

Of especial interest, sociologically, is the fact, brought out by an examination of the schools of Amsterdam, that when the parents are higher up the social scale, the children's eyes become emmetropic much sooner than for the lower classes socially. And this, apparently, quite apart from any inducements to earlier study. It is also interesting that in the individual the tendency toward emmetropy is so strong that when the anatomical form of the eye is emmetropic and

the natural tonus of the ciliary muscles would consequently produce myopy, the natural tonic contraction disappears.

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Ueber Bewegungsnachbilder. A. BORSCHKE und L. HESCHELES. Zeitschrift für Psychologie und Physiologie der Sinnesorgane, Bd. 27. Pp. 387–402.

This is a report of experiments conducted in the Physiological Institute of the University of Vienna with a view to determining the rate of apparent movement in after-images resulting from the observation of moving objects. Two sets of rods—one upright with a horizontal movement, the other horizontal with a vertical movement—were observed through a circular opening in a screen. One set remained constant in regard to speed, number of rods, degree of illumination and time of exposure; the other varied. Two after-images were produced, each moving in a direction opposite to that of the real movement. These combined in a resultant image, the direction of which served as a basis to measure the speed of the variable component.

The experiments showed that the speed of the after-image is, within certain limits, directly proportional to the speed of the original movement; that it increases with the number of impressions received per second; and that it is affected by the brightness of the stimuli and by the length of the observation. The authors, however, call attention to the fact that their method of measuring the speed is valid for a few seconds only in each observation. Longer periods showed considerable fluctuation in the direction of the after-image and interfered with the determination of its speed.

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RHYTHM AND TIME.

The number of the *American Journal of Psychology* for January, 1902, contains three papers on time and rhythm which may well be reviewed together. The articles are as follows:

The Relation of the Fluctuations of Judgments in the Estimation of Time Intervals to Vaso-Motor Waves. Study from the Psychological Laboratory of the University of Michigan. H. C. STEVENS. Amer. Jour. of Psych., XIII., 1–27, January, 1902

Accuracy in the judgment of time intervals is shown by Mr. Stevens to apparently correlate with blood supply. The periods when

judgments are least accurate seem to coincide with a rise in the vaso-motor wave as recorded by a finger plethysmograph. There is thus demonstrated a tendency of the blood to flow away from the brain when this higher mental activity is at its ebb.

The numerous experimental difficulties in securing satisfactory records of the dilatation of blood vessels simultaneously with psychic events gives this work of Mr. Stevens considerable value. The article contains several plates in which a direct comparison may be made between the fluctuation of time judgment and the course of the vaso-motor waves. This method, however, Mr. Stevens admits is not so convincing as that of averaging the errors in judgment which are made at the crest of the vaso-motor waves and comparing them with errors made coincident with the trough of the waves. A table of averages for 321 judgments made on intervals varying from .18 to 2.4 seconds indicates greater errors at the crest of the waves. The table might have been improved by giving the variability of the averages.

A second part of the study deals with the amount of the errors in time estimation and notes their failure to follow Weber's law. So far as constant errors are concerned he finds intervals below .72 sec. overestimated. From that to 2.4 sec. underestimated. From 3.7 to 7.24 sec. overestimated. These tendencies agree with Mehner's observations.

Respiration was also recorded during the experiments, but Mr. Stevens concludes that it played no significant part in the judgments except for intervals of 3.7 sec. or longer, when he finds the termini of the intervals occurring at similar points on the breathing curves.

A Contribution to the Psychology of Rhythm. By CHARLES H. SEARS, Fellow in Psychology, Clark University. Am. Jour. of Psych., XIII., 28–61, January, 1902.

The article deals with the time element in instrumental music, and follows one contributed by Mr. Sears last year on a different phase of rhythm. For this investigation four musicians played on an organ parts of four well-known hymns. The keys of the organ were electrically connected with pens writing on a kymograph. The time record of the soprano part was thus recorded. An interesting feature of the paper is the electrotypes of the music scores giving below each note its value in hundredths of a second for each player. Tables are also given showing the average length of measures, of whole, half, quarter, eighth and dotted notes. A similar analysis is made of the

time relations of two selections played by a music box. The investigation follows somewhat the work of Binet and Courtier, but has more direct connection with actual musical composition.

Personal differences are so great and individual variation so uncertain that Mr. Sears finds it difficult to draw any general conclusions. It is clear that the notes do not follow the ratios represented by the written music. Accented notes, he finds, are lengthened, as has been noted by other investigators. There is a strong tendency to make the third note of a triplet the longest, and a slight tendency to lengthen the second over the first note.

Meumann's conjecture that one hand of a musician aids the other in preserving the time intervals is not borne out by a comparison of the tempo when only the air was played with than when both hands were used.

The method used in measuring the length of tones makes the results somewhat delusive. The records show that each tone usually began before the preceding tone ceased. Sometimes, however, there was an interval between the tones. In both cases Mr. Sears proceeded, as he says, 'on the assumption that each note was intended by the player to last until the following note was struck and no longer.' The overlap was therefore subtracted from the full time of the preceding note and the interval was added when it occurred. It is evident that under this method the durations of the tones shown in the tables was not the actual duration of the sounds. It would seem to be better to have measured the actual time which each key was held down. This would certainly have been nearer the actual psychical effect of the tones themselves.

Rhythm, Time and Number. ROBERT MACDOUGALL, New York University. Amer. Jour. of Psych., XIII., 88–97.

While this article is only critical and descriptive, it gains import from the knowledge that Professor MacDougall has recently carried on a lengthy empirical study of the rhythmic group which is to be published in the *Harvard Studies.*

Dr. MacDougall criticizes the statement of Sully that the accurate measurement of time intervals shows itself at its best in the perception of the rhythmic successions of verse and music. The opposite seems to be true from the experimental facts. An interval that is not part of a rhythmic succession is in general more accurately reproduced. The preservation of a rhythm depends not upon 'the accurate measurement of successive intervals of time * * * but the maintenance of proportionate relations among the successive groups.'

Another common belief, that motor accompaniment of regularly recurrent sensations tends to aid the estimation of their time values, is also combated by Dr. MacDougall. He claims that the process of motor adjustment either distracts the attention and thus injures the judgment, or else the material of judgment is transformed in value by being changed to agree with some rhythm of the organism.

Professor MacDougall agrees with those who make time estimation dependent on 'widespread tensions and releases in the organism. Moreover, he believes that this process cannot be 'localized in a single mechanism,' as Mach attempts to connect it with the ear.

In regard to number perception, attention is called to the experiment of Dietze that rhythmical integration is involved in the apprehension of serial impressions beyond about six members.

J. Burt Miner.

Columbia University.

EFFECTS OF ALCOHOL AND OF FASTING.

Ueber die Dauer der psychischen Alcoholwirkung. Ernst Ruedin.

Ueber die Beeinflussung geistiger Leistungen durch Hungern. Wilhelm Weygandt. Psychologische Arbeiten, Vierter Band, 1. Heft.

The entire number is given up to these two papers. The purpose of the first experiment was to make a further test on a number of individuals of the effect of a single dose of alcohol upon simple mental processes like choice reaction, associations, adding, and committing to memory. The experiments were made in the laboratory of Professor Kraepelin, and according to the methods well known to persons who have followed Professor Kraepelin's work. They were carried out upon three subjects for eight days and upon a fourth subject for eleven days. The effect of alcohol upon a single subject has been worked before so that the present study is an attempt to determine individual reactions to its effects and the duration of them. Tests were made in the morning at 9 o'clock, in the afternoon at 2:30 and in the evening at 9. The test of each mental process lasted for half an hour and five-minute pauses were allowed between the tests of the mental process. The order was association test, pause, adding, pause, and committing to memory. Only subject 4 took the choice reactions, and one hundred reactions constituted a test. The dose of alcohol was the amount contained in a half liter of Greek wine, 90 to 100 grams of absolute alcohol. It was taken by all subjects on the fourth

day of the experiments a half hour before the evening test. The evidence of the effect of the dose was found in the amount the regular practice increase from test to test was reduced in the tests following the drinking. The conclusions stated at the close of the paper are: (1) The effect of a large dose of alcohol of 90 to 100 grams upon four different persons shows great diversities in reference to direction, strength and duration. (2) The effect of alcohol consists in general in slower adding, difficulty of committing, shortening of choice time with increase of error reactions and finally in an increase of associations, especially those resting upon speech forms. Only with one subject was this last clear. (3) The duration of alcohol effects is generally from 12 to 24 hours, appearing in one case for 48 hours. The shortening of the choice time disappeared most quickly to give place then to a lengthening of the same with a continuation of error reactions. (4) The sensibility to alcohol is not alone dependent upon habituation to the poison, but it can be slight even after very long abstemiousness.

The second paper is devoted to a study of the influence exercised by fasting upon mental process. The method followed is somewhat similar to that followed in the alcohol research. Six reagents volunteered their services. All alcoholic and stimulating drinks were proscribed for several days before the experiments began and during the time they lasted. The tests were made upon the first day and this constituted the normal or control day for the series. On the second day all food was abstained from for twenty-four hours. In two series both food and drink were denied, and in one the time of fasting was lengthened to 48 hours and in another to 72 hours. The tests were continued for two days after the fasting to discover the duration of the effects, or after effects. With one reagent the experiments were continued for nine days, two days intervening between the periods of fasting, and with the other reagents the experiment lasted for only four days as indicated. The psycho-physical methods employed to test the effects of hunger were generally the space threshold for the cheek bone and the glabella, the time for 50 associations, the test of perception (Auffassungsfähigkeit) indicated by the reading time of 277 one-syllable words and 287 nonsense syllables pasted upon a revolving drum, the time for 200 choice reactions, continued associations for five minutes from a given word recorded by a stenographer, and a half hour devoted to learning nonsense syllables by heart. Not all reagents took the same tests in the same numbers or in the same order. The details with which the results are worked out are extremely complex and quite up to the standard of the German investigator.

The most important conclusions of the paper are: (1) Psychical deportment experiences a clear change during the abstinence from nourishment. (2) The effect is a sharply limited one in so far that some processes are much, others little and still others not at all, affected. (3) The capacity for perception or apprehension (Auffassungsfähigkeit) is not affected by hunger. (4) The conceivable connection in associative thinking is weakened; inner associations decrease; associations based upon use in speech increase; sound associations cease. The temporal flow of association is not changed. (5) Adding is measurably slowed. (6) The work of memorizing becomes continuously and clearly slower. This disturbance affects only attention and not the rapidity of speaking. (7) Choice reactions show a slight lengthening; the number of error reactions is somewhat increased in places. The effect of practice, mental fatigue, distraction, emotional excitability, are either only slightly or not at all affected by hunger. It is much to be regretted that so large a piece of work, covering as it does more than 130 pages, should not have yielded more definite results. Nothing new or very striking has come from either research communicated in this number, and they must be laid aside with some feeling of disappointment. They, however, emphasize the enormous difficulties that are encountered in work of this kind, and Professor Kraepelin is to be congratulated upon not having long since lost his patience and given up. The work is painstaking and careful and it is to be hoped that others will be incited to attack these same problems in different ways.

T. L. Bolton.

University of Nebraska.

NEW BOOKS.

Development and Evolution. James Mark Baldwin. New York and London, The Macmillan Company. 1902. Pp. xvi + 395.

Elements of Experimental Phonetics. Edward Wheeler Scripture. New York and London, Charles Scribner's Sons. 1902. Pp. xvi + 627, and 26 plates.

Vorträge über Descendenztheorie. August Weismann. Jena, Fischer. 1902. Vol. I., pp. xii + 456; Vol. II., pp. vi + 462.

Die Grundsätze und das Wesen des Unendlichen in der Mathematik und Philosophie. Kurt Gessler. Leipzig, Teubner, 1902. Pp. viii + 417.

Experimental Sociology, Descriptive and Analytical. FRANCES A. KELLOR. New York and London, The Macmillan Company. 1901. Pp. xvi + 316.

Causeries Psychologiques. J. J. VAN BIERVLIET. Paris, Alcan.

NOTES.

DR. W. L. BRYAN has been elected President of Indiana University.

THE State University of Iowa has created a chair of psychology and elected to it Dr. C. E. Seashore, at present assistant professor in philosophy. Dr. Seashore took his doctor's degree at Yale in 1895, and was assistant in the Psychological Laboratory from 1895 to 1897.

W. S. JOHNSON, Ph.D. (Yale, '98), has been elected to the chair of philosophy and pedagogy in the University of Arkansas.

MR. J. H. BAIR, who last year held a scholarship in psychology at Columbia University, has been appointed assistant in anthropology. Mr. Elmer E. Jones, who also held a scholarship in psychology, has been elected professor of psychology and education in the State Normal School of Virginia.

A LABORATORY of experimental psychology will be opened next winter at King's College, London. It will be under the general supervision of the professor of physiology, Dr. Haliburton, and the special conduct will be entrusted to Dr. W. G. Smith, formerly of Smith College, Northampton, Mass.

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THE PSYCHOLOGICAL REVIEW.

A BIOLOGICAL VIEW OF PERCEPTION.

BY DR. THADDEUS L. BOLTON,

University of Nebraska.

Our purpose in the following pages is to revise one of the old categories of psychology in the light of some ideas borrowed from biological study. The inapplicability of the old categories to modern research has long been felt. The terms memory, association, attention, perception, etc., have served the purposes and methods of introspective psychology pretty well; and while they are not entirely satisfactory and applicable to experimental and comparative methods, they cannot be dispensed with right away, and besides no satisfactory substitutes have been offered for any of them. The chief and most serious objection to them is that they do not all refer with equal clearness to concrete realities which every observer can discover and identify. It requires too often judgment and careful interpretation to assign a certain phenomenon to one category rather than to another, and furthermore some phenomena of mental life persistently refuse to fall under any of the older categories. The almost complete failure of experimental studies along the old hypotheses, such as attention, reasoning, conscience, etc., would seem to indicate that these terms do not or cannot refer to uniform realities; they overlap one another and each seems to cut the grain of intellectual life at a different angle. What one excludes may not be included in any or all the others.

We shall submit the term perception to a short study to show that a large part of that which enters into every perceptive act

has been generally overlooked. The common view of perception held by most psychologists is something like this: A percept of an object is constituted of all the different sensations from all the various sense organs to which it appeals. The percept is a complex of sensations arising in connection with brain processes, both those directly initiated by the afferent currents and those aroused by these. The orange is the classical illustration in English as the sausage is in German. It is said that the orange has color, form, taste, smell, touch, etc. Now while this theory is not very critical and leaves many points of interest untouched, we are led to believe that these different sensory effects are immediately discriminated on the presentation of the object to the different organs of sense. No attempt will be made to deny that the percept is a complex of all these different sensations, but it is contended that these are the most superficial as well as the most obvious elements that enter into the percept. They are all the results of direct afferent nerve currents excited by the object, but they are, however, not the only or most important elements in the percept. There is in addition to them a much larger number of elements in every percept which are the results of afferent currents from the organism itself and these constitute the background or the framework in which these direct afferent effects inhere, so to speak. The older authors have recognized both an active and a passive part in all perception or have allowed that perception might be either passive or active, active perception being the more sure, accurate and discriminating with regard to its object. In active perception consciousness was said to go out toward the objects; it reacted upon them. In this reaction of consciousness all the associates of the elements presented by the direct afferent currents came up to interpret that which was directly presented. Some writers speak of presentative and representative elements in percepts. We propose to study more minutely from a comparative point of view this reaction upon the object and the associates which come up to interpret and illuminate what is immediately offered. The part that the back stroke plays in perception needs to be pointed out and emphasized—that which comes into the percept

by the way of the back door. Psychology is much indebted to Professor James for prying open the back door and to Morgan and others for propping it open permanently. It may in time be necessary to caution ourselves that the psychological tabernacle still has a front entrance, but until the back way is better known the caution is scarcely needed.

Our thesis is: Perception is an attitude toward an object as well as a complex of sensations, the attitude being characteristic of the object. To gain an understanding of what is meant by an attitude toward an object, we must trace perception down through the animal series to its earliest biological beginnings. In that way we shall see it gradually fading into pure automatic and instinctive performances. Perception reduced to its lowest terms is an act. The study of perception becomes synonymous with a study of animal activity. All perception below the level of conscious reflection amounts simply to a way of acting. In so far then as animals perceive objects they act in definite ways towards them. These various definite ways in which animals act towards objects are known as instincts, and objects to which animals do not possess any kind of instinctive or inborn method of acting are perforce unperceived by them. Those performances of animal life which are rigorously determined and carried out irresistibly in the presence of definite objects, always in the same way, are inchoate perceptions. Without doubt they are to be found only in the lowest forms of animal life, where perception is then synonymous with instinct. In perceiving the object the animal acts towards it, and even though we come up in the animal scale as far as man this fact of acting towards objects still enters largely into the perception of them. Perception in man must be considered only as a much modified and complex method of adjusting himself to his environment. The lower forms of life must act in the way they do, otherwise they are cut off. But even in the higher forms there still survives bodily action. We might almost paraphrase Professor James' sentence and say: 'Objects are presented to us, we act in determinate manners towards them and thus perceive them.' Objects to the lower forms of life, carrying with them as they do possibilities of life and death, are for this

reason emotional interests and these find their reality in the animal's acts. Only those objects with life and death possibilities for the lower forms of life, and those with pleasure and pain possibilities for the higher forms, have interests and are acted towards—consequently perceived.

Consciousness, if there be any in animals of low degrees of development, is without functional importance and can be left out of consideration. Professor Morgan allows that it may exist and calls it epiphenomenon. When, however, the reaction which an object provokes in an animal is imperfect and can be improved by successive trials, or when the instinctive performance may be modified by experience, which is practically the same thing as by repeated trials, consciousness comes to have functional value and the material it uses to modify the performance is presented by the currents that flow backward from the organism during activity and are initiated by the movements the organism makes. They are not the results of direct afferent currents started by the object that is present to the senses. The object provokes the movement and this in turn supplies through the back door the data which consciousness uses in the modification or perfection of the movement itself. The nervous processes excited by the object are perfectly continuous and joined in a causal series with those that provoke the movements which are allowed in all forms of life to be the animal's response to the object exciting the first processes in the series. The function of consciousness is nowhere to interfere or interpolate itself into this causal series, which is complete without it. It is a misconception of consciousness to suppose that it works in that way. We are far from denying to consciousness functional value and by no means do we do so when we declare that it can never interpolate itself between two nervous processes in a series which would be imperfect and disjointed without it. The idea that consciousness is a psychical minister to join in holy and functional wedlock forever nervous processes which could otherwise occur synchronously, rather than causally, is most certainly erroneous, even if it has gained wide currency. Now let us choose as an illustration of the way in which consciousness

modifies or perfects a movement the case of a newly hatched chick learning to discriminate foods.

It will be admitted that the first peck is purely automatic and is initiated by a moving particle. If every moving particle were good for food and there were no more than enough to meet the needs of the chick, it would peck automatically every moving particle presented, and, the supply and demand being equal, the chick could go on through life without the need of any conscious experience whatever. There would be no perception other than that mentioned above — instinctive performance. But there are two kinds of food, let us say, sweet and bitter, which differ characteristically by forms or colors. The sweet food is pecked and swallowed. The bitter is pecked and rejected after it excites in the mouth the bitter taste. The rejecting movement is provoked by the stimulus in the mouth. After a few trials this kind of food is not even pecked. That which at its first presentation evoked a pecking movement no longer does it; the object incites the animal to pass along. What we are contending for is this: that the elements which have entered into the chick's consciousness to modify its activity toward the bitter food have come chiefly through the back door. The significance of the bitter taste is to be found in the fact that it excited the rejecting movement. Simply as taste the bitter food could have no meaning for the animal, except the animal show some response to it by bodily movement. The currents that flow inward from the organism as a result of its activity initiate conscious processes which become associated with the conscious processes aroused by the afferent currents started by the object as presented to sight, so that when the object is presented again, instead of provoking the pecking movement, the head is rather drawn back and the animal passes on. It is an association or fusion of nervous processes primarily, and this means fusion of conscious states that accompany them. Bitter food is then to this animal the to-be-avoided-and-passed-by.

In this we find a clue to the meaning of all perception. Objects, in so far as they are objects at all, are things to be acted upon. The movements that they provoke are the significant phenomena for consciousness and without provoking motor re-

sponses objects cannot enter into conscious experience. To the automatic frog the snake is the to-be-screamed-at-and-jumped-away-from, while on the other hand, the fly is the to-be-snapped-at. What the object means, then, may be stated by describing the activity of the animal. It is not the color and form of the snake or of the fly that are of chief significance to the frog, but the color and form of the snake as provoking the screaming and jumping-away movement and of the fly as provoking the snapping-at movement which are significant to the frog consciousness. The biological history of color perception is something very similar. Inchoate colors are reactions. A piece of red flannel or other small red objects held down before the frog will excite him to snap, and even though the frog may be frightfully torn by a hook concealed in it, he does not desist. He may be snared several times in succession unless some other circumstance arise to provoke the fear reaction and flight. A moving red color of not too great size means the snapping movement. Red color, or 'the red rag,' for another reason, elicits an entirely different movement from the bull, and a still different one from the turkey gobbler. The red color of an artificial flower will entice certain bees to seek honey. In all these cases the real object, as it appears to human consciousness, remains unperceived. The flower which should, according to the analysts, be smell and taste as well as form and color is acted towards in the same way as if it possessed all the requisites to human consciousness. The consciousnesses of these different animals must be different as their bodily reactions to the colors are different. Over-fine psychologizing might find that the animal had taken one of the concomitants of the object for the whole object, overlooking all the others, and acted accordingly; but it is far simpler and, we believe, more correct to say that a certain stimulus, which is here a moving colored form, is followed by a given response, and that the stimulus gets its significance in the action it provokes.

Instead of children perceiving objects and describing objects in terms of their direct afferent effects, they choose to speak in terms of activity. The colt is said to be 'what runs behind the carriage' and a pig becomes 'what drinks its milk all up.' A

stump speech was described as ' when a man stands up and says a whole lot of things what nobody understands.' The pig is not a combination of colors and form, as we might be led to believe by the synthetic psychologists; it is a moving active thing. The child and the common man of the world use such terms chosen from their experience as have meaning for them; such terms are their own acts. They understand them through the avenue of the back stroke. The scientific man without a doubt finds it better suited to his purposes to describe objects in their direct sensory effects.

Wild animals, deer for instance, may be approached by hunters directly in front when it is possible to float down upon them in a boat so that there is no lateral movement. Just so long as the stimulus does not shift its position upon the sensory surface and thus avoids exciting a reaction towards adjustment, the object remains unperceived. If there be lateral movement, the eye must follow, and this becomes the key that unlocks the movements for flight. A mental state of fear in the deer would have no meaning; it could not be significant except it led directly to flight. The hunter is the to-be-fled-from, and he is nothing else to the deer. The deer has no other attitude towards him and he can have no other so long as he lives in the brush and is hunted.

In vision, perception of form arises through the backward flow from the ocular muscles as they contract and relax to move the eye so that the different points in the outline may fall successively upon the fovea. The recent experiments upon illusions of sight leave little doubt upon this point. Tactual space perception must be explained in a similar way. Differences in location upon the skin are felt as differences in tendencies to movement in relocating points touched.

Until the animal must act differently towards different individuals of the same species, there is really no need for other elements than those supplied by way of the back stroke. In the lowest instinctive and automatic performances the action is identical with the perception; but as we rise higher, the reactions are less and less rigorously determined. A given object is not always responded to in precisely the same way, a greater

variety of objects may be reacted to, and objects differing somewhat in detail may provoke the same and different responses. Practical necessities might then arise for giving definite responses to the slight differences in detail. The lowest animals perceive few objects, that is, they discriminate few. With the growth in multiplicity of instinctive reactions goes growth in discriminative power. Discrimination is shown by different reactions to the objects discriminated. The feeling of difference — the shock of difference, as Professor James puts it — arises from a difference in the bodily reverberations which objects severally excite. Psychical units or mind atoms, if they exist at all, are bodily changes of determinate sorts. That which first enters into conscious experience as perception arises not in connection with the direct afferent currents, but in connection with the backward flow from the motor response. The back-stroke effects come first, and only as discrimination grows and rises to higher importance do the direct afferent effects increase in significance; but they are always bound up with back-stroke effects. The appreciation of direct afferent affects is dependent always upon the increasing variety of motor responses which objects come to provoke in us. Discrimination, then, proceeds not by direct analysis of immediate presentations but through differences in the motor activities which objects excite. The striking difference between the inexperienced animals of the lowest and highest species is this: In the lowest species the objects that are responded to at all provoke definitely fixed and appropriate movements and other objects may not affect the animal's conduct in the least. In the highest species a greater variety of objects provoke responses which are in a large measure random and apparently purposeless. Experience works towards the selection from among these random movements those that are appropriate and serve some purpose, and by repetition makes them stereotyped and definitely fixed. The slighter differences in objects for which, on account of the rigidly determined character and limited number of their reactions, the lower species could show no appreciation, would provoke various random movements in the higher. Those individuals in whom the smallest differences in objects would arouse

the most varied responses would stand the greatest chances of finding a movement that would be appropriate to the differences, and thus gain an advantage over their less plastic fellows and become, through survival, the progenitors of the future generations.

The method of discrimination here offered can be seen more clearly by illustration. A child mistakes a goat for a dog, approaches it too incautiously and is butted over. The general form of four legs, a body, a head and a tail is alike in both and is the exciting cause for the movement of approach on the part of the child. The horns, which hitherto have remained unperceived, become associated with the painful feelings from the disastrous overthrow which is followed by flight, so that the goat no longer incites the child to approach but to retreat. Both goat and dog may now provoke flight until some quality of the dog excites again the movements to approach.

This failure to discriminate may be seen again in the identification of snow and sugar by another child, and here it looks like a recognition of likeness. The child's experience may be conceived to be something like this: The attendant tells the child that what is before him is sugar, and the child observing the object imitates the sound of the voice and says 'sugar.' In subsequent experience the sight of whiteness provokes the muscular contractions which result in the production of the sound 'sugar.' Again the eye falls upon something white—whiteness—and by a reflex mechanism the muscles contract and the sound 'sugar' is uttered. But this time it is the whiteness of snow that has provoked the utterance. Let it be that the child is corrected and told to say 'snow.' Subsequently he will say snow or sugar when whiteness is presented until some new experience is had. Sugar may be offered him at the same time that he is told that it is sugar. It is tasted, so that hereafter the word calls up the tasting movements. Snow is presented and tasted at the same time the word is pronounced. If the two present no objective differences in granulation to the eye, they must be tasted before they can be discriminated. The smaller and finer grains of the one will sooner or later call up the sugar taste, and the flakes of the other the chill reaction, when they

have several times been experienced together, and thus the discrimination will be accomplished.

The need for an enormous variety of bodily responses correspondent and commensurate with the differences among things that must be discriminated was met by the rise of language. The savage who has met a snake or a bear and was frightened and made to tremble thereby, comes home and acquaints his tribesmen by a weakened trembling of his members and a few movements of his hands. So, too, when he tells of the visit of another to his camp he gives first the tribal symbol of his visitor and then passes one hand above the other to indicate the entrance into his tepee and so on through the narrative. The higher civilized man substitutes conventional movements of his speech organs for the conventional movements which make up sign language, sign language being in large part a pantomime in conventional forms of real bodily activities which objects have excited. Thus each increase in the capacity for the discrimination of differences in direct afferent effects was made possible by the fact that bodily movements in the form of speech reactions were excited by the stimuli. Every sensory effect is bound up in a motor, or, as Professor Dewey puts it, sensation is sensation in so far as it is sensation producing motion. For the sensory effect to become apparent it must arouse movement — a perception reaction.

Defective sense perception on the part of children suffering from uncomplicated microcephaly must be interpreted as due more to a lack of proper muscular control for the sense organ. The eyes can be moved freely but objects presented to them do not provoke the usual reaction of fixation. The eyes cannot be controlled, because the nervous structures through which the afferent currents started by the object must pass are so imperfectly developed that these currents do not awaken the reflexes for adjusting the eyes to the object presented. A bitter substance like arrow-root or an irritating one like turpentine is swallowed when placed in the mouth because they do not awaken the usual rejecting movement. The swallowing reflexes being the primordial and fundamental, and hence more easily awakened, are aroused by the substances placed upon the

tongue. The inattention of idiots which is so frequently spoken of — the impossibility of holding the attention upon an object — must be interpreted as an imperfection in the mechanism by which in the normal objects adjust the sense organ so as to take full account of all the qualities presented by the objects. Perception is made possible by a series of reflexes which are successively awakened by an object. When the organism is not sufficiently developed to make possible the perception reflexes, objects are not attended to — they are not perceived.

Passive, bloodless contemplation or perception, which would mean immediate appreciation of direct sensory effects free from back-stroke effects, is less common than we suppose. The dispassionate scientist and the calm, cold, reasoning logician are more often fictions of the litterateur; the experimenter and the arm-chair occupant are animated objects, and, whether you will believe it or not, their perceptions, like all perceptions, are little more than refined emotions; and, thanks to Professor James, most psychologists are coming to believe that emotions get in by the back door. The scientist and philosopher find interests and wax warm and exultant over their perceptions which get their deepest reality and significance in the overflows which they excite. It is the same in everyday affairs. We might speak of a rose as of rose-feeling, of tobacco as of tobacco-interest, or of a beefsteak as of beefsteak-enthusiasm. To perceive a beefsteak it is not enough simply to note the form and color, to smell the odor and recall the taste, but it is as well to feel the bodily effects which the beefsteak makes upon us. The beefsteak is a thing to go into raptures over as well as to taste and smell and see; there is a genuine beefsteak bodily reverberation which is an inseparable part of the perception of it. As the proof of the pudding is said to be in the eating, so the perception of the beefsteak is in a certain sense in the eating. We might go still farther and say that the reality of all perceptions is in large part in the acting. How persistent and compelling is the bodily reverberation in perception may be seen in tasting a bitter substance like wormwood or in smelling a pungent substance like ammonia.

Our thesis then is this: All that objects mean to us is

largely due to the sensations that flow backward from the bodily reverberations they excite directly in us. Perception is an attitude towards the objects perceived. Perceptions grow out of those primitive ways of reacting towards objects which are both emotions and instincts. Mental life is constituted by adding to the direct afferent effects the interpretation which the back stroke gives. Only in this way can the emotional interests attached to objects be understood. The older view has held that emotional interests are grafted on to the combinations of sensations that have entered into the perceptions. This view would turn the order about. Emotional interests as determinate ways of acting come first, and perceptions are refinements and differentiations of these. The method of origin and development is the same in both. Scientific psychologists proceeding by the methods of analysis have penetrated only far enough to discover the direct afferent or sensory effects which enter into our perceptions. A vague and ill-defined theory of mind which is very suggestive of the Platonic doctrine of ideas seems to be implied in the view of perception arrived at by the analytical psychologists. The theory is something like this: Presented objects awaken mental processes which immediately and without further reference construct out of the data an image which is allowed to be a percept. On the contrary, mind is to be regarded as an outgrowth of conduct, a superior and more direct means of adjusting the organism to the environment—it is teleological.

STUDIES FROM THE PSYCHOLOGICAL LABORATORY OF THE UNIVERSITY OF CALIFORNIA.

COMMUNICATED BY PROFESSOR GEORGE M. STRATTON.

VI. 'Geometric–Optical' Illusions in Touch.

BY DR. ALICE ROBERTSON.

The interest which attaches to experiments upon the so-called 'geometric-optical' illusions, viz., the investigation of our perception of space, is not lessened when the investigation is carried into the tactual field. The experiments recorded in the following pages constitute an attempt to investigate, by touch alone, certain geometrical figures which present well-known optical illusions. Since sight and touch are so closely related, and since our theories of space perception are based in the main upon optical phenomena, the following observations may serve to test some of these theories. For example, from his study of reversible perspective, Thiéry[1] arrives at the conclusion that all optical illusion is due to the perspective in any given figure, whether consciously or unconsciously perceived. According to this observer, the convergence or divergence of lines produces in us an effect of depth, or of foreshortening, so that small angles are only larger ones interpreted perspectively, and an object seen near the apex of an angle seems larger than one at its opening, because it appears to be further away, and we connect distance with larger size. It is obvious that the tactual perception of plane figures, the mere contact of the fingers or of the hand upon any part of a flat surface, can produce no effect of perspective. When, however, it is found that illusion remains, serious doubt is cast upon the importance of perspective, even in the sight illusion.

In considering what figures are suitable for experimentation in the tactual field, it is clear that not all figures which pro-

[1] *Phil. Stud.*, XI., pp. 307 and 603, XII., p. 67.

duce an effect upon sight can be used. Simple figures, those containing but few lines, are best adapted to this purpose. If the figure is composed of many lines, a blur of sensations is received, and, as would be said in microscopy, it is difficult to get a sharp definition. The apparatus which was used in the following experiments consisted of black cardboard in which the figures were pricked with a fine cambric needle, the prickings being placed so close together that they could not tactually be distinguished as separate points. Or, in a few cases, the shape of the figure to be experimented with was made by pasting narrow strips upon a larger piece of cardboard, and this outline was either explored by the tips of the fingers, or the hand as a whole was passed over the figure.

Throughout these experiments, active touch has been employed. The rapidity of movement, the amount of pressure exerted, and the portion of the hand which received the sensation, make marked differences in some cases in the amount of illusion. Sometimes an illusion which existed in a very marked degree when the hand as a whole was passed over the figure, became almost inappreciable if the finger-tips were freely used to explore the contour. In other cases the illusion remained whichever method was adopted. As a rule, the hand has been passed rather lightly and somewhat rapidly over the figure, and the judgment has been recorded either in words or in a drawing of the object as it was perceived by the tactual sense.

The agreement or divergence between the illusions of touch and of sight afford a wide basis of classification for the experiments here described. In very few cases only can the tactual illusion be said to be merely in the same, or in a reverse direction from that which is found in sight. Other phenomena of illusion also appear, *e. g.*, illusions of curvature where lines are straight, or illusions of greater length or height where no difference exists in reality. Generally speaking, however, the whole set of experiments is divisible into two classes. The first includes those figures in which the illusion follows the same direction as that of sight. The second includes those figures which afford an illusion in the opposite direction. Other phenomena connected with these figures will be noticed in the description.

Class I.

The experiments which fall under the first class consist of a miscellaneous group of minor illusions which are common and well known in the field of sight. The purpose has not been to make a complete investigation of the phenomena revealed here, but merely to find out whether illusion exists, and, if so, to what extent it resembles the visual phenomena. These figures were presented from time to time to several subjects, no attempt being made, except in one or two cases, to vary the conditions.

1. *Müller-Lyer Illusion.*—In this well-known figure a marked tactual illusion exists. For purposes of experiment the oblique lines at the extremities of the horizontals were not joined close to the latter. Space enough was left so that the ends of the horizontals could be distinctly felt. In every case illusion

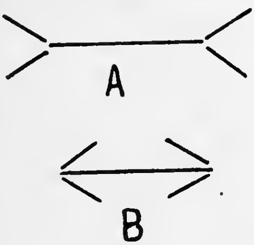

Fig. 1.

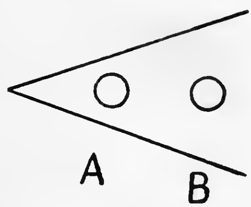

Fig. 2.

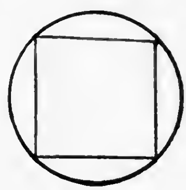

Fig. 3.

in regard to the length of the line was very apparent. It was in the same direction as that found in sight, but greatly intensified. That is, when compared by the sense of touch alone, *A* (Fig. 1) seemed not only longer than *B*, but the difference in length between them seemed much greater than appears to sight.

2. *Illusion of Convergent Lines.*—Experimentation upon a suitable figure of the pattern represented in the drawing gave perfectly definite and unvarying results. When the hand is passed over the figure and the sizes of the two circles are compared, that one (*A*) which is in the apex of the angle seems the larger—a result similar to that which is found in sight. The result in question seems to be due to a blending, to a certain degree, of the sides of the angle with the periphery of the circle and an interpretation of this as meaning that the circle *A* is larger than *B*, *B* being relatively uninfluenced by the lines

near it. If this be true, then the apparent size of *A* relative to *B* should change with a change in its position relative to the apex of the angle. This supposition seems to be confirmed by a few experiments conducted for the purpose of testing it.

3. *Perception of Angles.*—In the optical illusion presented by Fig. 3, the circle seems to be flattened somewhat where it touches the corners of the square, while the sides of the latter are very slightly bent inward. The same phenomena greatly accentuated appear also in the tactual illusion. In experimenting with this figure, subjects were requested not to explore the contour with the figure-tips. A record of the impression received by passing the hand back and forth over the figure as a whole, was made in drawing by each subject, and samples of the data obtained from two subjects, *S* and *N*, are given below (Figs. 4 and 5).

Fig. 4 represents the impression which subject *S* received

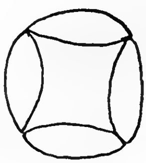

FIG. 4.

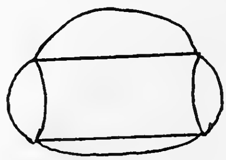

FIG. 5.

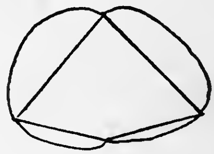

FIG. 6.

when the hand was passed over the figures as a whole in any direction. The sides of the square curved inward, and the periphery of the circle seemed to be divided into distinct segments, which flattened very much as their extremities approached the corners of the square.

Fig. 5 represents the impression received by subject *N* of the same figure. In this case the hand was passed from right to left or *vice versa* across the figure as a whole, at which time the square lengthened horizontally, the shorter sides only seeming to curve slightly inward. The circle seemed to be an ellipse somewhat flattened at the corners of the inner rectangle. When the figure was turned through 45°, and the hand was moved as before from right to left and back again, then the square became a flattened diamond shape, and the circle an ellipse somewhat flattened at the corners of the inclosed figure

(Fig. 6.) To this subject, 'horizontal' distances, that is, distances right and left, seem distinctly longer than equal vertical ones. Also, the upper part of a figure which is felt by the hand as a whole, usually seems higher and more distinct than the lower part of a symmetrical figure. Thus, in the two positions recorded above (Figs. 5 and 6) the curve of the ellipse is higher above than below, as is also the point of the diamond in Fig. 6.

4. *Illusions of Contour.*—A tactual illusion similar to that

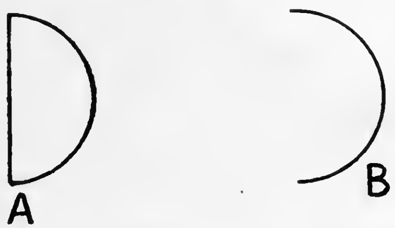

FIG. 7.

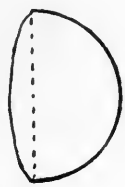

FIG. 8.

which is found in sight appears also when the hand is passed over two semicircles, the one closed and the other open, as represented in the drawing (Fig. 7). In this case the arc of the open semicircle seems to flatten out and to become the arc of a larger circle. Besides this illusion in contour another one appears in *A* which is not observed in sight. When the contour of *A* is perceived by the hand as a whole, the first impression is that of a figure composed of two curves, one of which is flatter than the other (Fig. 8). The curve of the arc of the circle seems to impress itself upon the chord and it appears to bulge slightly.

Similarly, if the two squares *A* and *B* (Fig. 9) are compared in the manner above described for the semicircles, the open figure *B* will seem the larger. In some cases *A* is felt as a square, *B* as a rectangle whose longer sides are horizontal. In other cases both *A* and *B* seem to be lengthened rectangles, *B* seeming the longer of the two. In the comparison of the semicircles and of the squares, the illusion in each case corresponds to what we find in sight, and probably for a similar reason, viz., the inclusion within the figure of some of the outside free space.

5. *Ring Segments.*—When the two segments (Fig. 10), which are objectively equal, are compared by touch, an illusion similar in direction to that of sight is very evident. Not only is a tactual illusion apparent when the segments are objectively equal, but also when the upper segment is actually larger than the lower, and when to sight no illusion whatever exists. That is, it is found by experimentation that, if two segments are compared in which the inner curve of the upper segment is equal to the upper curve of the lower (Fig. 11), a tactual illusion is apparent in a larger number of cases. In Fig. 10, 80% of the judgments obtained from five persons were in accord with the ordinary visual illusion, that is, *A* seemed smaller than *B*. At the same time, in the other 20% of the judgments, *A* was con-

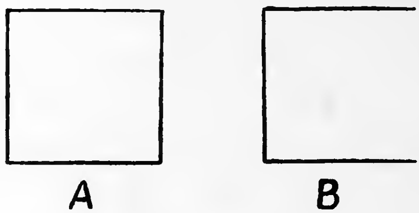

FIG. 9.

sidered either larger or equal to *B*, or the subject was in doubt. The evidence for illusion in this figure is by no means so conclusive, so unvarying in its effect on the tactual sense, as it is in vision. On the other hand, it is remarkable that in the unequal segments represented in Fig. 11, so large a percentage of judgments should give evidence of a tactual illusion. The larger size of *A* in Fig. 11 is very evident to the eye, yet when the comparison is made by touch, in 42% of the judgments *A* is considered smaller or equal in size to *B*. If the cards are turned at right angles, and the segments are compared in this position, the errors in judgment are increased. In the case of Fig. 10, the increase is not large, 81% of the judgments are in favor of the smaller size of *A*, while for Fig. 11, 56% of the judgments are that *A* is either smaller than *B* or equal to it. The error in these figures seems to indicate that the tactual comparison of the two segments becomes a comparison of the lengths of the two more closely approximated curves, rather

than a comparison of the size of the segments as a whole. This is thought to be the reason why an increase of errors occurs when the cards, and consequently the segments, are turned at right angles to the positions represented in Figs. 10 and 11. In this position the oblique sides of the segments are brought directly under the fingers, and hence come into more prominent notice. The tips of the fingers naturally follow the slanted edge of the upper or right-hand segment, and thus they are brought some distance within the slanted edge of the lower, or left-hand segment, and the former is considered the smaller.

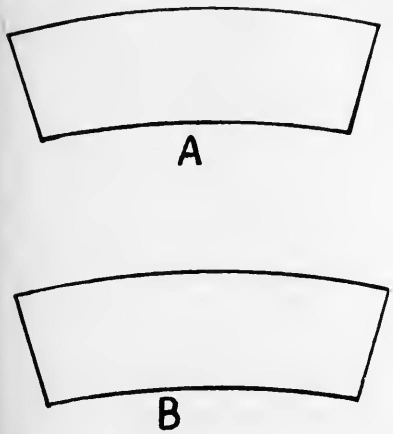

FIG. 10.

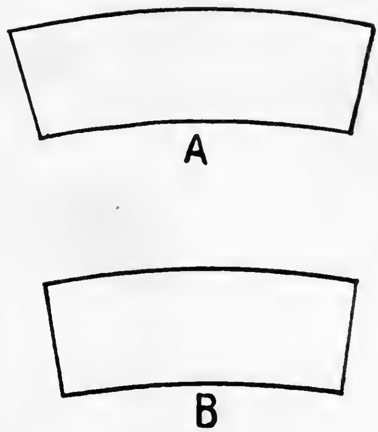

FIG. 11.

In each of the preceding figures certain tactual illusions occur which are in the same direction as those which appear to the eye and which seem analogous to the optical illusions. As has been said, these optical phenomena have been ascribed to perspective as the primary cause; but in the experiments here presented perspective cannot enter, and yet the results are the same. While it does not necessarily follow that the phenomena of sight and touch should be referred to the same cause, yet the results here obtained are thought to diminish the force of perspective as a fundamental cause of illusion even in sight.

Class II.

1. *A Quantitative Comparison of Lines of a Varying Number of Interruptions with an Uninterrupted Line of Standard Length.*—The experiment here described is intended to investigate the phenomena which appear when lines variously interrupted are compared, by the sense of touch alone, with an uninterrupted line. The results show a reversal of the illusion which appears in the visual field, and instead of an overestimation of the interrupted extents such as takes place in vision, there is underestimation, *i. e.*, the interrupted lines seem shorter than they actually are.

The apparatus which was used consisted of a number of cards, made of black cardboard, 27 cm. long by 13 cm. wide, in the center of which the lines were pricked. Four kinds of lines were used. First, a plain, uninterrupted, smoothly pricked line (————). Secondly, a line similar to the preceding but having its extremities defined by a short line at right angles (|————|). Thirdly, a line defined at each extremity by cross lines and also divided in the middle (|——|——|); and fourthly, a line divided in a similar manner into sixths (|–|–|–|–|–|–|). The cards thus fall into four groups, each group consisting of a series of lines varying in length from 7 cm. to 15 cm. The lines varied by steps of 5 mm. throughout that portion of the series where the difference was actually found to be difficult to perceive. The lengths of the lines composing a series, then, were as follows: 7, 8, 9, 9.5, 10, 10.5, 11, 11.5, 12, 12.5, 13, 14, 15 centimeters. The standard line with which all comparisons were made, was a plain unbroken line (————) ten centimeters in length.

In conducting the experiment the subject was seated with closed eyes before a table of convenient height, upon which his whole arm rested comfortably. The standard card was first presented to him, and then the other cards of any particular set, one after the other, were laid below it for his comparison. He was permitted to pass the hand back and forth from the card to be compared to the standard, and *vice versa*, as often as he pleased before he gave his judgment. The right hand was always used, and no restrictions were placed upon him as to

what part of the hand should receive the stimulation. Sometimes the tips of the fingers alone were used, again the portion of the hand just below the finger-tips. In giving a judgment the subject was asked to state whether the line seemed longer or shorter than the standard or equal to it. A series consisted of an ascending and descending portion, and an equal number of each began with the shortest line and proceeded gradually to the longest, and *vice versa*.

Ten such double series were obtained from each of three subjects, *B*, *S* and *N*, the results of which are shown in Table I. As a matter of fact, series were obtained from many more persons, and the results in many cases were much more striking than those which are here presented. The experiments here recorded are, however, in every way the most systematic and trustworthy. In the table each value for the upper (U. T.) and lower (L. T.) threshold is an average of twenty single determinations, and the equality point (E. P.) and mean variation (M. V.) are an average of forty determinations.

Examination of the table of thresholds shows much individual variation in the ability to estimate the differences between the various lines. All show underestimation, that is, the line which is compared, when really equal to a standard, seems shorter than the standard, even in Group I. There seems to be much difficulty for all subjects to discriminate in the case of this group. In the

TABLE I.

TABLE OF TACTUAL THRESHOLDS AND MEAN VARIATION FOR THE FOUR GROUPS OF LINES.

	Group I.				Group II.				Group III.				Group IV.			
	U. T.	L. T.	E. P.	M. V.	U. T.	L. T.	E. P.	M. V.	U. T.	L. T.	E. P.	M. V.	U. T.	L. T.	E. P.	M. V.
B	10.4	10.4	10.4	.12	10.9	10.8	10.8	.25	11.3	11.2	11.2	.28	11.4	11.2	11.3	.43
S	11.2	10.8	11	.24	12.3	11	11.6	.43	12.1	11.6	11.8	.37	12.5	11.8	12.1	.53
N	10.7	10.6	10.6	.40	11.8	11.7	11.7	.34	11.2	11.2	11.2	.37	12.6	10.5	10.5	.38

cases here reported one subject, *S*, shows an underestimation amounting to 1 cm., the other two subjects each average about one half a centimeter of error. The amount of error in the case of subject *S* is always large, but increases at an even rate with the number of interruptions, so that the line with the greatest number of interruptions seems in his case to be the shortest. For subject *B* the amount of underestimation is somewhat less in each case, but it proceeds at the same even pace, and a line much divided seems shorter than one objectively equal but undivided. The case is somewhat different for *N*. For this subject the effect of the limiting lines at the extremities in Group II. is marked by a sudden increase in the amount of underestimation. The compared line in Group II. seems to be shortest of all, while the compared lines in Groups III. and IV. relatively lengthen. But even with this subject the line containing the greater number of interruptions is equal to the uninterrupted line and not longer, as is the case in sight.

From the data furnished by these experiments, we may conclude that when a line ten centimeters in length, definitely marked at its extremities, and with or without interruptions in its length, is compared with a plain unbroken line objectively equal to it, it appears shorter to the tactual sense, or is underestimated. In general, this result agrees with the conclusion at which Professor Parrish[1] arrived in his investigation of similar phenomena with passive touch. He used lines 64 mm. long, all being marked at their extremities and variously interrupted in their extents. He considers that the results which he obtained clearly point to a reversal of the optical phenomena. Dr. Dresslar,[2] on the other hand, concludes from experiments which he conducted with both active and passive touch, that the tactual illusion follows the same direction as the illusion of sight. A study of the data of the latter's experiments, however, given in Tables I. and II., pp. 334, 335, of his article, suggests that perhaps a transition-point from under- to overestimation may be found in them, between the long and the short

[1] *Amer. Jour. of Psy.*, VI., p. 514.
[2] *Amer. Jour. of Psy.*, VI., p. 314.

interrupted intervals. Certainly in Table II., in which the judgments are given upon longer lines (5 to 16 cm.), there is a decided falling off of the relative number of judgments in favor of the greater length of the filled space. Indeed, the writer himself remarks on page 337, that 'when the spaces to be compared are more than 10 cm. in length, the illusion does not hold so steadily.' In fact, from about 10 cm. on, the illusion tends to take the opposite direction from that which appeared below that length and from that which appears in the visual field.

From the results of a few tentative experiments upon short interrupted intervals, an analogy between our sensations of touch and our perception of time is suggested. It is well known that time of a given length, but interrupted at regular intervals, seems within certain limits to be shorter than an equal unbroken period. It has been found, however, that for very short intervals the illusion changes in character, and such periods when interrupted at regular intervals appear to be longer than an equal unbroken time.[1]

For the purpose of investigating this matter experimentally a number of cards were prepared, on each of which there was marked off a short space defined by limiting lines. The spaces formed a series and were respectively 8, 9, 10, 11 and 12 mm. wide, defined at each extremity by a pricked line one centimeter in length. The standard for comparison consisted of a space 10 mm. wide which was broken at regular intervals by five lines (| | | | |). Thirty series (150 judgments) were obtained from each of the three subjects, *B*, *S* and *N*. The method of right and wrong cases was adopted. The cards to be compared were presented in no regular order, but were shuffled at intervals. A parallel experiment, thirty series for each person, was also carried out, the standard in this experiment being an unbroken space of 10 mm. long (| |).

The results of the two experiments are given in Table II. The data for both experiments from each subject are placed one below the other so that their comparison may be more easily made. The upper line of the table gives the widths of the unbroken spaces, or the variables which were compared with

[1] Meumann, *Phil. Stud.*, IX., p. 264, and XII., p. 127.

TABLE II.

Sub.	No. of Exp.	Character of Standard.	Distribution of Judgments when Variable was: 8 mm.					9 mm.					10 mm.					11 mm.					12 mm.				
			L.	S.	E.	E. or S.	D.	L.	S.	E.	E. or S.	D.	L.	S.	E.	E. or S.	D.	L.	S.	E.	E. or S.	D.	L.	S.	E.	E. or S.	D.
B	30			30					30					28	1	1		2	16	12			10	8	11	1	
	30			28	2			2	18	4	1	4	7	11	5	2	4	25	1	4			28	1	1		
N	30		1	27	2			1	26	3			9	17	4			18	9	3			30				
	30		1	29				2	19	9			15	5	10			29		1			30				
S	30		6	19	5			5	17	8			9	11	8		2	17	8	4		1	24	1	3	2	
	30		1	23	5		2	2	19	7		2	7	10	8		5	24	1	5			29			1	

the two standards. The letters L, S, E, E or S, and D in the second line, stand respectively for the judgments longer, shorter, equal, equal or shorter, and doubtful. The number of judgments of each kind is arranged in two lines for each subject, the upper line giving the judgment of comparison with the broken standard (| | | | |), the lower line, those with the unbroken one (| |). Thus, taking the first two lines which represent the judgments given by the subject *B*, we see that an unbroken interval of 8 and 9 mm., when compared with the standard, is thought to be shorter each time. The comparison of an unbroken interval 10 mm. in length with an equal broken interval gives 28 judgments of shorter, and the unbroken spaces of 11 and 12 mm. give a predominance of the judgments of shorter and equal. Contrasting these results with those obtained from comparison with the unbroken standard, it will be seen that for this subject there is ample evidence for an overestimation of intervals where the standard is 10 mm. in length. In these sets the 10 mm. and 11 mm. intervals are the most instructive. Considering those for subject *N*, the 10 mm. unbroken interval is considered shorter than an equal broken interval 17 times, and equal only 4 times; while the 11 mm. interval is thought to be longer 18 times, and either shorter or equal 12 times. When the same two intervals are compared with the unbroken standard, the judgments of shorter for the 10 mm. interval diminish, while those for 11 mm. show almost no illusion.

In the case of subject *S*, the results are not so conclusive. Unlike the first two subjects, he knew the purpose of the experiment and felt, himself, that this knowledge was a difficulty in the way of giving a ready judgment. When the unbroken standard was used for comparison, there is a slight decrease in the judgments of 'shorter' and an increase of 'doubtful' and 'equal' for the 10 mm. interval; while for 11 mm. there is a decided increase of judgments of 'longer' and 'equal' with a decrease of 'shorter.' These results, when considered by themselves, may be said to indicate a tendency toward the overestimation of interrupted intervals. Taken in connection with those given by the two other subjects, there is a strong indica-

tion that in the tactual field a general law holds true, viz., that long interrupted extents are underestimated, short ones overestimated.[1]

The underestimation of interrupted extents by the tactual

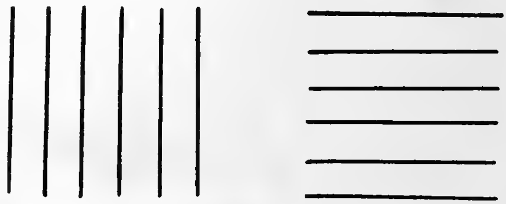

Fig. 12.

sense is also shown in the comparison of squares which are composed of either horizontal or vertical lines. When squares similar to *A* and *B* (Fig. 12), whose sides are 10 cm. long, are pricked in cardboard and are felt by running the hand as a whole over them from right to left, or vice versa, then an illusion appears in the reverse direction from that perceived by sight. *A*

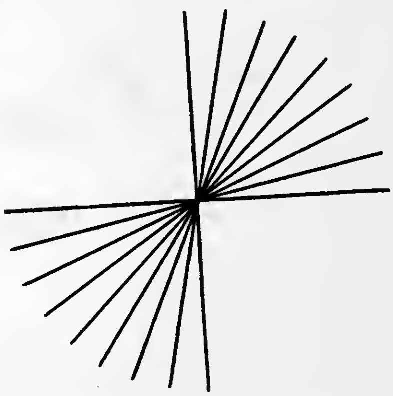

Fig. 13.

seems to lengthen so that it appears to be a rectangle whose horizontal sides are longer and whose vertical sides are shorter. On the other hand, *B* seems to shorten horizontally and to lengthen vertically. Similar phenomena occur in the figure which

[1] This is in accord with the results of Professor Rieber, to be published in the forthcoming volume of *Harvard Studies*. Dr. Rieber has very kindly placed his results in outline at our disposal for comparison.

represents alternate quadrants of interrupted and uninterrupted extents (Fig. 13). When the hand is passed over such a figure, the open or uninterrupted quadrant seems decidedly the larger. While this may be taken as added proof that interrupted extents are underestimated, yet the apparently very large size of the open quadrant is probably due in part to the inclusion by the hand of much of the surrounding free space. The arc through which the hand sweeps in passing over the open quadrant, not being well defined, seems greater and may in reality be greater than that through which it passes when feeling the quadrant filled with radiating lines. The tactual illusion in this case is analogous to that which is found in sight, although in an opposite direction. For, while in sight the uninterrupted quadrant seems smaller than the filled one, this is doubtless partly due to the fact that we compare the *arc* of the 'filled' quadrant, *i. e.*, the ends of the radiating lines, with the *chord* of the arc of the adjoining open quadrant. The optical illusion, then, is partly due to the *leaving out* of some of the space which belongs to the open quadrant. The tactual illusion, on the other hand, is heightened by the *taking in* of additional space.

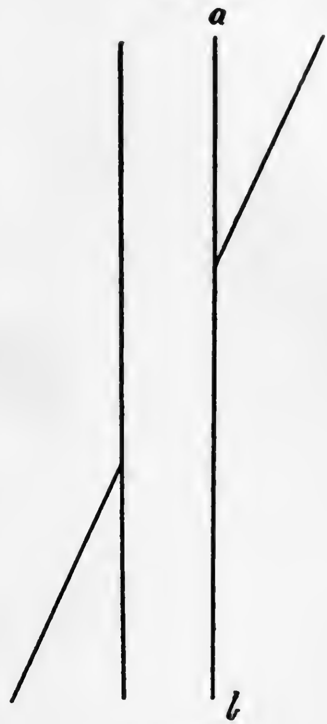

FIG. 14.

2. *Poggendorff Figure.*—The Poggendorff figure has long been a favorite subject for investigation as an optical illusion. Many theories center about it and the closely related Zöllner figure, but so far as I know no attempt has been made to investigate the tactual phenomena connected with it. A few tentative experiments gave very constant and somewhat surprising results. For whether the subject was or was not already acquainted with the optical illusion which appears in this figure,

whether he had or had not previously seen the figure which was presented to him, the illusion was very apparent, but in a reverse direction from that which appears to the eye. In the optical illusion which this figure presents, the lower left half of the oblique line appears to be *too low*, and not directly continuous with the upper right half (Fig. 14). In the tactual illusion, on the contrary, the lower left half of the oblique seems *too high* to be considered a continuation of the upper right half of the same line.[1] Moreover, the amount of displacement in the tactual illusion seems to be much greater than in the visual. It was, therefore, thought worth while to conduct a set of experiments for the purpose of making some quantitative determinations. A figure was constructed having one part of the oblique line movable, so that the amount of displacement could be easily measured. The verticals were placed 30 mm. apart, and the oblique crossed them at an angle of 40°. At no time was the subject permitted to see the figure. The sheet of cardboard upon which it was constructed was placed before the subject so that his arm was at right angles to the transverse line over which his hand should pass. It was found that if this line were slowly and carefully traced with the finger-tips, the illusion either did not appear or was very faint. In all cases it was required to judge of the direct continuation of the two parts of the oblique line by passing the flat of the hand over it, either alternately up and down, or in one direction only, as the subject desired. As a matter of fact, most persons settled into the one method of passing the hand from right to left downward over the line.

The experiment was conducted with four persons, *B*, *S*, *D* and *A*. From each of these, five double series were obtained. A descending series began with the transverse lines really continuous, although in no case did they seem so to the subject.

[1] Professor Dresslar proposes an explanation for an illusion of displacement of crossed lines which he considers may explain the optical illusion in the Poggendorff figure, and which by implication, at least, seems to be intended as an explanation of the tactual illusion in this figure. In my own experiments with the Poggendorff figure, the tactual illusion is shown to be in an opposite direction from that which appears in sight, and this fact would seem not only to render Dr. Dresslar's proposed explanation inadequate for the illusion in touch but also to throw doubt upon that offered for sight. See *Amer. Jour. of Psy.*, VI., p. 275.

The movable side was then moved downward by steps of 2 or 2.5 mm. to the point where the two halves of the transverse line seemed to the subject to be continuous, and then below that

TABLE III.

AVERAGE THRESHOLDS AND MEAN VARIATION.

Subject.	Upper Ave. Thr.	Lower Ave. Thr.	General Average.	Mean Variation.
B	— 15.2 mm.	— 24.1	— 19.6	4.6
S	— 27.2	— 30.1	— 28.6	8.3
D	— 24.0	— 30.1	— 27.1	4.5
A	— 45.0	— 50.3	— 47.7	9.0
Aver.	— 27.8	— 33.6	— 30.7	6.6

point until the left side was clearly too low. An ascending series retraced these steps to zero. Every such series, of course, gave two thresholds. In tabulating the data a calculated equality point was found of all the upper thresholds for the upper limit of continuity, or upper threshold. In a similar way the lower limit of continuity was found. Table III. gives the results which were obtained from each of the subjects according to this method. In the table the minus sign signifies the distance of displacement downward, measured along the line *ab* in Fig. 14.

FIG. 15.

It will be seen that the mean upper threshold for all four subjects is — 27.8 mm., the mean lower — 33.6 mm., thus giving a general average of about — 31 mm. That is, the lower left-hand portion of the transverse line must be moved downward 31 mm. on an average before the two halves seem to be continuous. If we contrast this number with that which was obtained by Burmester[1] in his investigation of the optical illusion in the Poggendorff figure, we find a very

[1] *Zeitschrift für Psychologie,* XII., p. 369.

wide difference in the amount of displacement which the two senses of sight and touch reveal. With a breadth of 30 mm. between the verticals and an angle of 40°, this investigator found an average of -5.09 mm. as the amount of displacement which was required to make the lines look continuous when the figure was in a vertical position.

With some persons the two halves of the oblique line felt as if they were parts of parallels, but the lower left-hand portion seemed to be at a higher level than the upper right-hand portion. It seemed to be the unanimous opinion of those who experienced this illusion that the feeling of 'too high' was due in large part to the vertical parallels. These lines, it was thought, guided the hand downward, below the point where it should cross the space between the verticals, and in order to reach the lower portion of the transverse line an actual upward effort was necessary. In order to test the influence of the verticals upon the illusion a second figure was made, omitting the vertical lines altogether (Fig. 15). This, like Fig. 14, was made with one side movable, so that the amount of displacement could be measured. Five double series were obtained from each of three subjects, *B*, *S* and *D*, and the upper and lower limits noted as before. The data are tabulated in Table IV., and some very interesting results appear. Thus, in the

Table IV.

Table of Average Thresholds and Mean Variation When Verticals are Omitted.

Subj.	Aver. Upper Thr.	Aver. Lower Thr.	General Average.	Mean Variation.
B	$+5.3$	$+2.6$	$+4.0$	3.95
S	-7.2	-8.3	-7.7	3.49
D	$+.7$	-3.7	-1.5	2.5

case of *B* the direction of illusion changes, and the lines seem continuous at some point above where they really are so. The judgments given by subject *D* vacillate above and below the zero point, and if we take the average of the two thresholds to be the point where the two lines would seem continuous to this subject, we find it to be -1.5 mm. In this case, then, the illusion is practically nothing. With subject *S* the threshold

always falls below zero, on an average -7.7 mm. From the data it seems clear that in the absence of the verticals the tactual illusion is very greatly weakened and almost nil. In experimenting with a similar figure, Burmester found that the optical illusion was much weakened and took an opposite direction. This experimenter was at the same time his own subject, so that it is possible that if he had operated with other persons, individual differences would have appeared as they do here.

An attempt was made to counteract the influence of the verticals by filling in the space between the end of the oblique lines with lines running horizontally. The vertical parallels are of course suggested by the ends of the horizontals, but since the lines in the transverse direction are the more prominent, it was thought that they would exert the greater influence and weaken the illusion, or perhaps reverse its direction. Several figures were made, in all of which the inclination of the oblique line remained constant, 30°, but in which both the lengths of the horizontal lines and their distance apart varied. The results of experimentation indicated a decided weakening of the illusion, but in no case was reversal obtained.

In whatever position the Poggendorff figure in its normal form was laid, illusion was apparent. The amount of pressure exerted, and the rapidity of movement, seem to have an effect upon the amount of apparent displacement. Thus, in the case of a figure in which the oblique lines were fixed, it was found that, with hard pressure and rapid movement, the lower left-hand line seemed too high; whereas, with the same pressure approximately, and slow movement, the two halves of the oblique line seemed to be continuous.

Various theories suggest themselves as a partial explanation of the tactual illusion which is exhibited by the Poggendorff figure. That the verticals in some way influence the amount and direction of illusion in both sight and touch is obvious enough. To some persons they seemed to exert a mechanical influence in actually leading the hand astray, so that in passing downward from the upper right-hand oblique to the lower left, an upward effort is necessary in order to find the lower part of the line,

leading the subject to consider that that portion of the line is on a higher level. An attempt was made to get tracings of the path which the hand described in passing from one portion of the oblique to the other. This was done by placing strips of smoked paper in the path of a wire which was attached to the hand. Thus, in one instance when the lower oblique was moved downward 10 mm., in passing the hand from above downward the two halves seemed continuous, while in passing from below upward the lower left part of the oblique seemed too high; in these two instances, however, no difference can be detected in the two tracings. That part of the curve which represents the path of the hand between the verticals is almost a straight line in both cases, and each is the normal and regular continuation of the first part of the tracing. In another instance, the two obliques were separated by a vertical distance of 18 mm. To the subject the lines seemed continuous with both the upward and downward movement, and the smoked paper tracings were two perfectly even and smooth, almost parallel lines. In a third instance the obliques were separated by a vertical distance of 23 mm. At this point they seemed continuous to the subject, while the tracing shows many irregularities. These, however, occur, not only in the space between the verticals, but throughout the lines, and may be ascribed to natural tremors of the hand. There is no evidence that there is an actual upward movement of the hand corresponding to the effort which some subjects believed they felt.

Data obtained from figures similar to those used in these experiments have afforded a basis for opposing theories of space perception. The perspective theory of Thiéry has already been mentioned. He sees in the Poggendorff figure also a definite perspective effect which is the cause of the apparent shifting of the two halves of the oblique line. Professor Wundt[1] considers that the cause of the optical illusion in this figure is the overestimation of the acute angles. The perspective effect, he maintains, appears only when one fixates a point monocularly, at which time the displacement of the oblique

[1] 'Die geometrisch-optischen Täuschungen,' *Abhandl. d. königl. sächs. Gesellsch. d. Wissensch.*, XLII.

lines disappears entirely. The 'ästhetisch-mechanische' theory of Lipps,[1] offered first as an explanation of spatial form, has later been applied to geometrical optical illusions. Among other figures, this writer discusses the Poggendorff figure. He applies to it his theory of the interaction of opposing forces, and considers that it suggests the action of the two forces of gravity and vertical extension. The oblique line represents a force approaching, but not attaining, verticality. In the struggle this force is regarded as the primary activity, and as primary activity is overestimated.

We have, then, at least three explanations of the phenomena of the Poggendorff figure. It is here shown how the same figure may give very different sensations to the skin. How are the facts to be reconciled? If the optical phenomena of this figure are due to the overestimation of the bending of the obliques away from the vertical, according to Lipps, or if they are due to an overestimation of the small angles, according to Wundt, why should not these causes operate in the field of touch, and, if they do, why should opposite effects be produced upon the tactual sense? Likewise, no explanation for the optical illusion in the ring segments satisfies the touch phenomena in the same figures. Here again, perspective effects and the overestimation of small angles are offered in explanation. But the illusion persists in touch, when none is apparent to sight, and when all perspective and almost all angle effect is lost completely. The data which are afforded by experiments in the tactual field suggest a revision of the theories so far offered for spatial illusions in general. These theories are in the main founded upon optical phenomena. From what appears in the tactual field it is reasonable to suppose that further study may assist in elucidating this very complex and difficult problem.

[1] 'Raumästhetik und geometrisch-optische Täuschungen,' *Schriften der Gesell. für psy. Forschung*, Vol. II., p. 295.

FEELING AND SELF-AWARENESS.

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Of all facts of the mind having important bearings upon life and art, none are by nature so hard to approach as feelings. We overlook them, because we look beyond them. They stand in such relation to self that while we have a feeling for every phase of the mental life, we cannot, without the most careful effort, form an idea of feeling.

I.

Thought, when viewed from within, refers to an object other than itself; feeling, when looked at from the same point of view, lacks objective reference; thought and feeling are often thus contrasted. Feeling is always felt, and can be symbolically represented and thought; but to do so, one must form an image of himself in some emotional attitude. The usual method of portraying thought and feeling by setting them over against each other is valid, however, only for purposes of introduction. To bring the matter to exact statement, one must add that thoughts are also felt. Every thought is part of that continuum of immediate experience called feeling. They are not entirely different: they unite in every moment of mental life.

It is not easy to say where, among mental states, objective reference begins and ends. When a little girl cried because a pile of books, in tumbling down against a table, must 'hurt'; when, in an absent-minded hour at the beach, the tide comes in and wets one's coat, awakening a feeling of discomfort and semi-alarm which he thinks of as belonging to the things around him instead of to himself; when a well-known professor, after a wearisome journey, sees himself reflected in a mirror at the other end of the omnibus he is entering, and thinks it is 'some broken-down school-teacher' entering from the opposite end; when a religious devotee, filled with mystery and dependence,

feels united in being with the object which he worships — in these and in innumerable similar experiences, objects are clothed with feelings very much as the sky is clothed with blue and the stars with space.

This objective reference of feeling is not unusual. At one of the earlier stages of mental development, where the difference between 'outer' and 'inner' experiences is not yet realized, this projection of feeling is the rule. The mind, moreover, never outgrows it. In all cases of intense or uncontrolled feeling, objects are clothed with the inner mood. Nature always wears the colors of spirit: to the melancholy she is melancholy, just as to the jaundiced she is yellow. A landscape, looked at through a yellow glass, is 'cheerful'; looked at through a blue glass, it is subdued and still. Everyone has known the mood when 'evermore most weary seemed the sea, weary the oar, weary the wandering fields of barren foam,' and the utter soul tedium when 'hateful is the dark-blue sky, vaulted o'er the dark-blue sea.' All mysticism is marked by this projection of feeling. In a recent book called 'The Story of My Heart,' scores of passages like the following occur: "I spoke to the sea: though so far, in my mind I saw it, green at the rim of the earth and blue in deeper ocean: I desired to have its strength, its mystery and glory." The mystic doctrine of 'the ground of the soul,' that 'apex of the mind' which is not merely united to God but one with Him, of which Eckhart says, 'Diess Fünkelein, das ist Gott,' involves the identification of feeling with its object. 'I live, yet not I, but God in me'; 'I and my Father are one.' In the mystic experience the difference between that which is outer and that which is inner is broken down and borne away, so that feeling acquires an objective reference and thought a subjective reference which are quite contrary to the usual contrast between them. The difference between thought and feeling in this matter of objective reference is, from a psychological point of view, relative, not absolute; it requires an inference to determine the relation of feeling and the self; we take up with abstractions whenever we part company with that unity of thought and feeling which every moment of mental life illustrates.

II.

Thought is often set over against feeling and sensation as though it were less immediate than they are. On the contrary, however, a thought that is not felt (that is, immediate) and a sensation or feeling that is not discriminated (that is, thought), are abstractions of the same order.[1] This popular opposition results from trying to portray thought, feeling and sensation without a self, experience without a subject. When we stop to think how the different elements of experience stand to each other, it becomes clear that as elements and ultimates they are all abstractions. In times of pleasure and desire, self feels itself one with its object (this, so far as the self is explicitly conscious of itself at all), and at times of pain and aversion it feels different from its object. To hate is to feel different from hate's object; love unites the self with its object. The closeness and inwardness of this felt unity of the self and the immediate object of its interest vary directly as pleasure and inversely as pain, and this relational feeling of unity belongs to the essence of attention, of thought, and of will. Out of it flow all motor processes of adjustment; when the self is wholly implicit, the process is reflex, and when explicit, voluntary. Moreover, motor processes not only may flow from this felt unity, they always arise whenever it is felt. In other words, self-consciousness is a synthesis of many elements, and every new object or image is felt to be either identical with or different from some or all of these elements; and upon this sameness or difference, felt as change (or its absence) in the content of self, depend the forms of interest, the forms of thought and the direction of motor adjustments.

Everyone knows by introspection that there is a difference between feeling, with its internal ebulitions, and an idea which, as an idea, does not involve self-awareness at all. Feelings are private: thoughts are shared or public. Whenever any experience is shared by others, it acquires an objective signification, which may or may not involve externality according as the experience is predominantly one of thought or one of feeling.

[1] Cf. A. F. Shand, 'Feeling and Thought,' *Mind*, Vol. VII., p. 477 ff.

Beauty, goodness and other sentiments are objective without being external to ourselves: they are all reflective and borrow a sort of publicity from the universality of the self-consciousness upon which they rest, a consciousness of the kind of beings to which the private self belongs. The experience of sentiment, however, remains always a private and personal experience: it is impossible to think of it as impersonal, as, for example, we sometimes (though very uncritically) think of a law of nature as impersonal. One's judgment involves a deference to the judgment of society which is not involved in the immediate experience of feeling. So far, all feeling is ours.

III.

Again, it is held that the difference between thought and feeling can be made out genetically, if not analytically — the doctrine of the 'relative priority of feeling.' According to this view thought is highly differentiated and integrated feeling. If feeling were prior to thought, however, we ought to be able to say what kind of feeling came first; and if none can be found which can be so regarded, why designate primitive consciousness as feeling any more than as thought or motor tendency? It is, in fact, impossible to find a pure feeling, or one out of which the cognitive and more complex forms of mental life have grown. In addition to feeling, awareness and motor tendency must be assumed — awareness of something and motor tendency in some direction. At any stage of organic growth at which psychic life can be said to begin, no doubt motor tendencies in harmony with the individual's welfare already exist; but they exist at first as inherited dispositions without ideas of acts, of ends, or of stimuli to either. About all that can be said about primitive consciousness is, a vague immediacy and awareness, as much feeling as thought, as much thought as feeling, fraught with motor tendencies which, when carried out in movements, result in the differentiation of both feeling and awareness.

We cannot say that primitive consciousness is pure feeling, without implying that, by reactions which are not conscious toward stimuli which are not discriminated or apprehended, feel-

ings are changed into ideas, thoughts and self-conscious movement — an implication which Spencer attempted, but failed, to substantiate. Feeling, as a sense of change, has played a large part in the discussion of mental growth, but a sense of change which involves no discrimination at all, being pure feeling, is unthinkable and, for the purposes of genetic theory, useless. What is a shock of change which does not contain an awareness of anything from which or of anything to which change takes place? The introspective differences between thought, feeling and motor tendency are irreducible; and a primitive consciousness that is one of these and not the other two is, to the writer at least, unthinkable.

On the other hand, once grant that early conscious experience is an awareness containing implicit, potential references to self and an object, together with motor tendencies and attitudes, and the exigencies of organic existence in a life-medium are such that differentiation of this primitive unity can be understood. Awareness of a pleasure-giving stimulus means focalization of consciousness upon it, tendency toward it, and movement, which brings more of the stimulus. Constantly occurring variations of movement result in variations of stimulus, variations of pleasure, repetitions or inhibitions of the variations, and so on: we need only to add memory, to see how a continual selection of those movements which bring more pleasure or less pain results. Growth, differentiation within the early unity without doing violence to it and only changing it to enrich and enlarge it, takes place naturally from the nature and conditions of life; and, we might add, this 'biological necessity' finds its positive condition and explanation in the primitive unity of awareness and feeling, rather than in the environment alone. For the individual himself, at the earlier stages of life, 'environment' does not exist, and nothing that does not exist for him can determine his mental growth.

From physiological theory we can derive much to substantiate the view that the experience of feeling is not prior to thought in the genesis of mind.[1] Feeling involves many ele-

[1] Stanley, 'Evolutionary Psychology of Feeling,' holds that feeling is primary, and that pain is the first form of feeling.

ments of the nervous system. The stimulation of sensory paths, reaching the cortex, starts motor discharges which affect the unstriped muscles of the vaso-motor system and certain secretory organs. The cortical motor centers (probably in the Rolandic region) discharge also (1) toward the frontal lobes which add (possibly, as Flechsig and others think) an affective tone to the emotion, and (2) downward toward the reflex centers of emotional 'expression,' resulting in changes in the processes of digestion, respiration and circulation. The muscular changes due to these motor impulses report to consciousness in changes of cœnæsthesia and in kinæsthetic sensations, the cortical centers of which discharge once more to the motor centers of the Rolandic region and these to the frontal lobes. The cells of the frontal lobes (possibly) discharge with another accompaniment of pleasure or pain which affects the centers of voluntary movement either positively or negatively. Of course physiology cannot show that this account is perfect: it cannot even show that the nervous system is necessary to the experience of feeling: but enough is known to make it probable that at the lowest stages of conscious life, feeling is vague, homogeneous and faint, much poorer than any feelings which we know.

IV.

Baldwin's dictionary defines feeling very broadly. "Consciousness as experiencing modifications abstracted from (1) the determination of objects and (2) the determination of action." If we abstract from the determination of objects and from the determination of action, what is left of any reflective experience is some form of self-awareness — some sense of well-being or ill-being, of activity or passivity. So interpreted, this account resembles Professor Wundt's definition of Gemüth: "The totality of states which we ascribe directly to some activity or passivity of our ego."[1] Of course the consciousness of self is not always clear and explicit: quite the contrary. No explicit awareness of the self, as distinct from the not-self, appears until comparatively late. At first, consciousness seems to be centered outward so that feelings are ascribed to objects; but,

[1] Wundt's 'Phys. Psych.,' 4th ed., II., p. 497.

implicitly at first and explicitly at the end of reflective experience, *feeling is always an attributive element in the consciousness of self.* It is true, we are constantly reading our emotions and simpler feelings into the objects before the attention, but such moments are unreflective.

V.

To this law that all feelings are attributive to one self, their unitary character is due. Empirical psychology frequently states the law of habit in a way which recognizes this truth.[1] Many thoughts, ideas and sensations may be presented to consciousness at any one moment, but only one feeling, or sum-total of feeling. The one feeling may contain many elements; one element or another may predominate at any given moment; and the change from one dominating element to another may take place very slowly or very rapidly. The self can, however, be in but one emotional state at a time. Cœnæsthesia illustrates the point. One or another element may predominate at a given moment, but one always feels well or ill or indifferent: he cannot feel all three at once. Moreover, the emotional effect of a series of experiences occupying successive moments of time but felt and thought as a whole, must be one. No matter how many and varied the partial emotional tendencies of the elements of one total experience may be, the emotional tone of the experience as a whole is always a unitary synthesis of the many partial tendencies.

Not only the unity of feeling, but also the feeling of unity in the objects of knowledge is traced by many psychologists to the oneness of the self that feels and possesses the interest. Feeling seems to be the great individualizer of things: love's objects are all unique. On the other hand, no object is unique which does not appeal to the passional nature, which is not an object of interest. It is a position which investigation strengthens, that unity and continuity of interest is present wherever an object is thus individualized. That conative unity and continuity have an 'altogether predominating importance' in the growth of intelligence, has been recently urged by Stout;[2] and conation is but

[1] Cf. Baldwin, 'Mental Development, Methods and Processes,' 1900, p. 216.
[2] Stout, 'Manual of Psychology,' London, 1899, p. 75.

directed feeling. Conation may be analyzed into a feeling of tendency toward or away from some object. Tendency is an irreducible feeling, and its direction toward or away from its object is probably determined by pleasure in the one case and pain in the other.[1] The feeling of tendency and pleasure-pain are never found entirely separate from each other, and yet they cannot be identified.

Professor Baldwin has emphasized the importance of motor habit in the unification of knowledge. "Association has, accordingly, a motor foundation from the first. The elements hold together in memory because they are used together in action. And as the action becomes one, but yet complex, so the mental content tends to become one, but yet complex also."[2] That this view is not, however, inconsistent with the one maintained here, is clear when we consider why things are used together in action. According to Professor Baldwin's view, the synthesis of pleasure and a given stimulus conditions the formation of motor habits. Unity, wholeness and integrity may be regarded, from this point of view, as feelings of relation based upon habitual motor tendencies: but it is due to feeling as a factor in imitation, according to Professor Baldwin, that motor habits are formed. Discriminations, apprehensions of differences, are due to reactions, are functions of movement; integrations, those habits of reacting which underlie the consciousness of unity, may be regarded as functions of feeling. A child's world is one and very simple; not a confusion of many things, but one thing, for lack of movement; reactions develop differences and discriminations.

The question why feeling unifies cannot be answered by saying that it is because the self which feels *is* one, without reasoning in a circle. If feeling were the great unifier, self would be one because felt as one. We are here face to face with a fact beyond which, so far as I see, we cannot go. Unity is simply the category of feeling, and feeling and self-consciousness are not to be separated even in thought. There is no

[1] On this point see Pfänder, 'Phänomenologie des Wollens,' Leipzig, 1900, p. 70.

[2] *Loc. cit.*, p. 310.

feeling without an either implicit or explicit self-awareness. Difficulties arise because the awareness of some object is always crowding self-awareness out of the mind, in so far as self-awareness is a matter of explicit discrimination. The idea of undiscriminated and unconscious self-feelings has suggested itself to many; but the notion has made more difficulties than it has ever overcome, and is inconsistent with the unity of feeling and thought emphasized above.[1]

Feeling individuates, we might say, because feeling is always a feeling of soul activity, and this is necessarily a one-to-one relation; but this is a repetition of the circle in thought, to which we referred above. We must give up all attempts to explain knowledge as the mere tool for the realization of certain states of feeling. Man needs whatever is necessary to complete himself, and knowledge certainly comes within this definition. A distinction must, however, be made between mediate and immediate needs, between those needs which are immediately felt as needs and those which are inferred from the whole course of history and from the nature of experience. Shortsightedness is the weakness of that utilitarian epistemology which insists that nothing is real which is not an immediate pleasure, and that no conception is true which does not serve as an instrument for the immediate satisfaction of man's practical needs. Reflection, based upon explicit self-consciousness, is an original function of spirit under which all immediate satisfactions are transformed. Man experiences an entire transvaluation of values when he passes from the immediate to the reflective stage of experience; he comes to realize that he needs whatever is consistent with the self. Self-consistency (in this sense) or an experience which is self-consistent, that is, complete and lacking nothing, is the ultimate object of all finite need; but it is an object which includes the subject, that is, the need itself. It is essential to this object that it be known as what it is; it is this knowledge which makes it what it is, and hence, this need of all needs can be adequately conceived only as a determination of reflection. Reflective knowledge is not so

[1] This seems to me a great difficulty in v. Feldegg's 'Philosophie des Gefühls'—an unpsychological use of the notion of unconscious feeling.

much an instrument for the satisfaction of need, as the architect who is at once both the builder of personal experience and the chief element in it. Mere feeling cannot give unity and individuality to such an activity, whether this be conceived as producer or as product; and I do not see how philosophy is to avoid simply inferring, and assuming at the same time, that spiritual activity involves a one-to-one relation between itself and its product, that is, a relation of self-consistency, a relation which is at the same time a relating activity, feeling itself to be such and realizing itself as such in a world of objects.

Objects, however, are universals: the only genuine particular is the this-now experience, and the this-now experience reduces at last to feeling, that is, to a self-conscious activity of spirit. Consequently, while it is not feeling that gives individuality to the objects of experience, it is nevertheless true that only those objects possess individuality which are felt. Strictly speaking, it is not the object but the activity that is felt; but it is never easy to say where activity stops and the object begins, and feelings are always being objectified. We speak of feeling a coin in the hand, confusing feeling with sensation, of course; but apart from the touch sensation, the experience *is* one of feeling in so far as it is an activity of the self, and it is certain that the coin possesses unity only so far as it is felt, that is, so far as it is an activity of the perceiving self.

VI.

Von Feldegg identifies the ego or soul with feeling, but thinks that soul-feeling was originally unconscious: in its inmost being it is always so. But what is a feeling or other mental state of which we are not conscious? How can the conscious be said to be an outgrowth of the unconscious? How can knowledge grow from mere feeling? And, does it mean anything at all to speak of a feeling which involves no discrimination?

The great name of Leibnitz, among others, gives dignity to this notion of unconscious consciousness, but we need not here repeat all the arguments which he advances in favor of it.[1] One will be sufficient for our present purpose. Stimuli must reach

[1] Cf. 'New Essay,' New York, 1896, p. 109 ff.

certain intensities before they result in sensations or feelings; but, he argues, the weaker intensities of stimuli must have some effect upon consciousness, as upon the opposite supposition their having any effect whatever becomes unintelligible. We must therefore distinguish between 'remarkable' or noticeable sensation and sensation in general, and hold that there is a vast realm of sensations of which we are never distinctly conscious. This argument, like others which the New Essay contains, assumes that sensation is a sort of thing or objective reality, independent of the self which experiences 'remarkable sensations,' and this assumption is one for which present-day psychology does not find justification. Leibnitz' view, moreover, logically implies that the self is, at the lowest stages of its development, an object of experience, and that it later becomes a subject. In more recent years, the ego has not unfrequently been spoken of as though it were at first an object of experience, then the unconscious subject of a purely objective experience, then the subject of a purely subjective experience, and finally the subject of a universal experience like that of the adult human being. But such a self is not psychological; and as a metaphysical conception it is replete with contradictions for the understanding. There is no genuine self except the self-conscious-of-its-relations-to-other-selves. Self-conscious reflection is for psychology the very essence of a self.

Ribot, quoting Spinoza, holds that awareness of the body and its processes is the soul or self.[1] Spinoza wrote, "The first element which constitutes the actual being of the human mind, is the idea of some particular thing actually existing." In Proposition Thirteen, "The object of the idea constituting the human mind is the body * * * and nothing else." Proposition Fourteen, "The human mind is capable of perceiving a great many things, and is so, in proportion as its body is capable of receiving a great number of impressions." All this is, however, a metaphysical deduction from the ideas upon which Spinoza's Ethics is founded, and I doubt whether Spinoza had in mind the same view of the soul as Ribot,[2] or ever once

[1] Ribot, 'Diseases of Personality,' Open Court Translation, p. 18.

[2] For further reasons for this doubt see below.

thought of that body of pathological phenomena upon the evidence of which Ribot makes the following assertions: "It is the organic sense, the sense of the body, usually vague and obscure, but at times very clear in all of us, that constitutes for each animal the basis of his psychic individuality. It is that 'principle of individuation' so much sought after by scholastic doctors; for directly or indirectly all rests upon it. We may regard it as highly probable that the farther we descend in the animal scale the more the sense of the body preponderates, down to the point where it becomes the entire psychic individuality. But in man and the higher animals the turbulent world of desires, passions, perceptions, images and ideas covers up this silent background. Except at intervals, it is forgotten, because it is unknown."

Ribot's conclusion is drawn from the interesting facts recorded in this work; but the number and novelty of the facts do not add to the logical weight of the argument. The mental effects of alcohol, of a blow on the head, of exhaustion and fatigue, of fever, and of many other well-known stresses point in the same direction as the less common facts which this book records. They all point to the notion which has for several years been the working basis of psychology, viz., the notion of a parallelism between bodily and mental states, a notion which applies to the awareness of self as well as to awareness of objects. Ribot's view, however, goes farther than this mere working hypothesis, holding that bodily states entirely determine mental states, that bodily sensations make up the sense of self, and that the lower animals which have larger bodies, and simpler ones, than man, are more self-conscious than man. On the contrary, the consciousness of self is not a bodily sensation; it is rather a product of social intercourse and appears only at an advanced stage of mental growth. Animals below man in the evolutionary series are not guided by a sense of the kind of beings they are, so far as one can see in their actions; nor are they more individual than man. All the evidence relating to the influence of a growing social consciousness on individuality, tells against this view. Self-consciousness is much more than a simple awareness of the

body; it is a concept, more or less adequate, of a class of beings into which the individual puts himself; and this the lower animals do not seem to possess.

The lower animals are probably not aware of themselves as subjects of experience at all. It may be true that they are more intensely and constantly aware of their bodies than man is aware of his body; but there exists a great difference between being aware of one's body and being self-conscious, between the child's awareness of his feet and hands, and the adult's awareness of himself as the subject of an experience into which feet and hands enter only as elements of content. Spinoza holds that the mind consists, not only in ideas of the body, but also in ideas of those ideas. In Proposition Twenty-two of Part Two, "The human mind perceives, not only the modifications of the body, but also the ideas of such modifications." In Proposition Twenty-nine and elsewhere, he speaks of 'the idea of the idea of each modification of the human body.' Spinoza recognizes degrees of self-awareness such as the immediate happiness of the child, the awareness which expresses itself in the clause 'I am happy,' then, 'I am aware that I am happy,' and lastly, 'I am aware that I am aware that I am happy.' The view of Ribot that self-awareness is awareness of the body, does not take this reflective form of self-awareness sufficiently into account.

Professor Dewey has pointed out[1] that 'the essential difference between me and thee' is the feeling that every consciousness is *my* consciousness. If it were a knowledge it would be universal and objective. Feeling, which, simply because it is individual and particular, cannot be defined, is the immediate sense of self-activity. Self, according to Professor Dewey, is activity. "Through its activity, the soul is; and feeling is the becoming conscious of its own being." Professor Wundt's view of the self is similar. The Actualitätsbegriff der Seele, of which Wundt is the champion, as opposed to the Substanzbegriff der Seele of which he is the foe, is the conception of an activity immediately aware of itself in feeling. Professor Wundt maintains that the soul cannot be adequately represented

[1] 'Psychology,' 1891, p. 247.

at all — first, because it is a subject and never an object, and secondly, because it is a feeling of a particular activity, while objects are universals. According to Professor Dewey, feeling unites the particular self to the universal implied in every instance of knowledge; the will translates the object by attention into terms of feeling. "From the standpoint of psychology, consciousness is always both objective and subjective, both universal and individual" (p. 24). "If we consider this activity in the value it has as manifesting to us something of the nature of the universe, it is knowledge; if we consider it in the value it has in the development of the self, it is feeling; if we consider it as an activity, including both the universal element which is its content and the individual from which it starts and to which it returns, it is will" (p. 22).

Feeling is always the feeling of an either implicit or explicit self, and feeling varies with the intensity or weakness, the success or failure, the tension or freedom of this activity. The view of Professor Wundt that feeling has three dimensions, viz., Lust-Unlust, Erregung-Beruhigung, Spannung-Lösung, is one for which there is much evidence.[1] Students of mental diseases frequently distinguish emotional conditions of heightened from those of lowered tension, the terms which they use indicating something more than variations of pleasure and pain. Moreover, introspective analysis is continually running across variations of feeling which are, with great difficulty, classed as hedonic. Exaltation, for example, is sometimes pleasant, as in moments of successful effort, but sometimes painful, as in insomnia and other mental disorders.

VII.

One's actual feelings with reference to self are closely connected with the content of the self notion at the various stages of its growth and can only be described in terms of this content. A large group of sensible elements, *inter alia*, enter into it — visual and tactual images of the body and its parts as distinct

[1] Cf. Max Brahn, 'Experimentelle Beiträge zur Gefühlslehre,' *Phil. Stud.*, Bd. XVIII., p. 127 ff. Brahn studies the pulse-variations as effected by feeling, and finds in them empirical confirmation of Wundt's view.

from and related to other objects in space; motor images of bodily movements, and the sense of motor potential; cœnæsthesia, and certain auditory, olfactory and gustatory sensations of the body which are always with us. These constitute what Wundt calls a 'permanent group of ideas' to which other elements of the complex self-idea are assimilated; they are the first elements of self-awareness. The different feelings which accompany the different sensations are, for our present purpose, not important; for many of them we have no descriptive terms or names. These sensations would not, however, become a consciousness of self at all, were they not feelings as well as sensations, and did not their feelings, like all feelings, contain an implicit self-awareness. The sense of well-or-ill-being must have marked the earliest awareness. As the powers of special sensation, dependent upon physiological structures (developed by use) unfold, the sensations are already saturated with feeling. "The point," says Höffding, "at which the ideas and their combinations obtain an influence over feeling cannot therefore be far from the beginning of conscious life, though this influence may not be plainly apparent until a later stage."[1] As perception grew into memory and ideation, as the various stimuli assailing the senses were remembered, recognized and grouped, as images became vague and general, the immediate sense of well-or-ill-being may have become anticipation, fear, and sorrow. Most likely fear came to play a predominating part at this stage.

What interests us most at this point is, first, the probability that the sense of well-or-ill-being must have been present along with sensation from the first, and, secondly, that although this immediate sense contained from the first an implicit self-reference, no consciousness of self as a subject of experience exists until a later time. Pleasure-pain is attributed to extra-corporal things in space, or to the body itself as a thing in a world of things. Self feelings of personal identity and personal agency depend for their rise upon various factors. Feelings, when some development in powers of sensation has been realized and when not due to strong or sudden stimuli, arise more slowly than

[1] Höffding, 'Outlines of Psychology,' New York, 1893, p. 233.

sensations; and analgesia may exist without anæsthesia, and vice versa, under the influence of drugs, extreme cold, hypnotism, etc. This may have aided in the process of self-differentiation.

The relation of opposition between self and every particular object, and the effort of an organism to adjust itself to new stimuli, involving as it does feelings of strain, of muscular co-ordination, of unified activity, must have contributed much to the growth of the self. Especially the effort toward adjustment to other organisms must have tended to develop a sense of adjustment which is potentially a sense of self-activity. The most varied and unpredictable stimuli in the environment of any organism are due to other organisms; they are the ever uncertain and mysterious things, but not wholly mysterious. The fact of character, or uniformity of uncertainty, must have called forth uniformities of expectation and adjustment, as Baldwin has urged. The effort of an individual to adjust himself to such uncertainties may tend to bring to a focus all the lines of tendency toward the thought of self. Effort is, moreover, a psychologically irreducible feeling and is capable of rising to an obtrusive intensity in consciousness. The memory that, of other organisms, some always flee, some always attack, while others always share, etc., must also contribute to the growth of the self-thought. Then there is the recognition that other organisms treat things and each other in a manner very familiar. The individual organism's actions are a problem to other organisms as truly as their actions are a problem to him, and his problem is the same as that of others. Imitation comes before self-consciousness, and the imitated adjustments of individuals to each other and to their common environment serve as aids to the generalizing process which results in the idea of a class of beings to which the individual himself belongs, and to the feeling of self-identity. Meanwhile, the fact of self-interest, not yet realized as self-interest, present in all feeling and directing all activities to the realization of self, obtrudes more and more, tending always to become a discrimination and an emotion. Finally, language, or some system of arbitrary symbols for the communication of thought and feeling, gives universality and consistency to the thought of self. The rise of uni-

versality and consistency seems to be one with the rise of language and dependent upon it.

All the influences tending to the growth of the self notion are covered by the term imitation as used by Tarde, Royce and Baldwin. Baldwin's Dictionary defines imitation as 'any repetition in thought, action or both which reinstates a copy.' Self-awareness must be conceived as such a copy. Except perhaps in its crudest elements, as vague bodily feeling, it is a function of social experiences: social experience emphasizes and defines it. When the self-idea first crosses the threshold of discrimination, it is an awareness of self as a being sustaining certain relations to other beings, of a being of instinct, impulse and convention (using this term in a very broad sense), of certain habitual attitudes and hedonic capacities, of a being who belongs to certain classes of beings, and whose subsistence as a member of this class and that consists in certain types of activity, thought and (consequently) feeling. Not that all this comes suddenly into consciousness — far from it. Every instance of social experience, of experience involving more than one organism, tends to emphasize some one or more of these elements; and there is no self-consciousness which is not a consciousness of the imitatively realized relations of self to other selves. There is no self-knowledge which does not involve feelings of particular relations of self to other selves, to nature or to reality; and it is a question whether nature and reality are not, for us, always, in so far as they are reflectively experienced, personal — whether our relations to them, so far as they are reflectively conceived, are not necessarily social in type. Self-awareness assimilates to itself, all along the process of its growth, all impulses and instincts, pleasure-pain capacities, ideas and motor attitudes; they are its content, while it is their form and unity. Self-feeling is never a feeling of self in general. No wraith of a general self is felt except as the object, never as the subject, of thought. Self-consciousness of the reflective type is paradoxical in so far as it is the consciousness of a particular-self-sustaining-constant-relations-to-other-selves. The self gets its reality from the immediacy of its own particular existence. Here, as always, it is the particular that is felt; self-feeling is

feeling of my particular self as doing, suffering or thinking some particular thing. Even in this case we are, however, dealing with a social product. The sense of personal identity is, as Ormond contends, a function of the social consciousness, and it is this sense of personal identity or selfhood which gives to the emotional life the reflective type of unity which marks all æsthetic, ethical and philosophical emotions.

VIII.

This is clearly manifest in the feelings of familiarity, generality and wholeness which enter into the reflective syntheses of memory, conception, judgment and inference. The discovery that, by the same law, vapor rises, rain falls, and the moon swings round the earth, awakens a strong emotion due to the variety of effective tendencies gathered into the one resulting mood, from the feelings of unity and generality which enter into the thought of these phenomena. The value of a work of art is said to depend upon the complexity of a unified series of images or perceptions, each of which possesses its own emotional tendency but so related to the series as a whole as not to disturb its dominant mood. If we ask ourselves why this is true, we recall that such syntheses are very useful and even necessary to the existence of mind and the mental life. These emotions are the premiums which nature has put upon singleness of view, upon economy of the attention, upon intellectual adaptation. If we further ask why singleness and simplicity of view please, no answer, except that reactions which conserve and enrich the self are always accompanied by pleasure, is possible. "To the question, why this unity pleases, no answer can be given except the fact itself."[1] Failure to realize mental simplicity and singleness, means failure of the mind itself. The unity demanded is unity of the mind with itself, self-consistency; and this is simply a demand for continuity and universality of experience, for an experience containing no want or incompleteness, no interruption or unresolved manifoldness. Unity, pleasure and ex-

[1] Ormond, 'Foundations of Knowledge,' 1900, p. 224.

istence come so near to each other here that to ask why unity pleases is tantamount to asking why unity is unity. The question attempts to apply the bare form of thought to itself.

It is sometimes said that the demand for unity in our world is a demand of the æsthetic consciousness. To the present writer it seems to be just as much a demand of the logical and of the ethical consciousnesses; indeed, is it not at last a demand of the self in all forms of its activity? Professor Ormond maintains that 'it finds its tap-root in the intuition of self-consciousness.' Moreover, there is a tendency in some writers to limit the æsthetic consciousness to the reflective experience of unity in the sphere of perception alone. Calkins[1] so defines æsthetic emotion. According to this usage, the 'associative connections, emotional revivals, volitional and ethical reverberations'[2] are not to be considered essential to the æsthetic experience itself. Hegel[3] maintained that the unity of nature and spirit which is immediately presented in beauty is always qualified by the sensuous externality which embodies it. Consequently, to speak of the beauty of truth, the beauty of holiness or the beauty of character is allowable only upon condition that we define truth, holiness and character, as some primitive peoples and advanced philosophers have tried to do, in terms of things of the objective world. If we accept this limitation of æsthetic experience to the consciousness of the unity of self with objects of perception and their representations in memory, it follows that beauty involves unity, but not that all unity is beauty or beautiful.

The passion for unity is to the mind what the old 'instinct of self-preservation' was considered to be to the organism — a passion for being and reality; and there is a purely formal point of view from which it may be said that what the demand for unity demands is at last only itself. Writers have maintained that this is a logical, others that it is an ethical, demand. Perhaps it were better to regard it as a trait of self-consciousness, rooted in reflective self-feeling, underlying and explaining the æsthetic, ethical and logical unities.

[1] 'Introduction to Psychology,' 1901, p. 278.

[2] Baldwin, 'Handbook: Feeling and Will,' 1891, p. 239.

[3] 'Philosophy of Mind,' Wallace tr., p. 169.

Emotions, in actual experience, are never general or abstract, but particular.[1] One often realizes, in looking back over a given series of past experiences, that he has enjoyed or suffered similarly throughout them all. I now recall that my emotional attitudes and tendencies were the same or similar throughout a series of many experiences and thus realize the 'spirit of a country,' the 'atmosphere of an author,' or the 'genius of a composer.' But these are neither 'abstract emotions' nor 'emotional abstracts' (if indeed there is any difference between these). The abstract thing is the thought of the emotional attitudes common to the entire series of particular experiences: emotion is always particular.[2] There are emotions peculiar to the logical functions, but not a logic of the emotions. Such an experience as the spirit of a country is a logical concept and, like other logical concepts, it embraces an image to which a feeling of generality attaches and into which it enters; but this feeling of generality is totally different from the 'spirit of the country,' being in quality like such concepts as landscape, garden, crowd, etc. The feeling of generality is distinctly *my* feeling, but it may be symbolized and made the object of thought. Moreover, owing to the social forms of consciousness, one reflects that there must *be* generality; one ejects generality into his thought of the world.

Apart from self-consciousness, emotions are abstractions; as such they are thoughts, not emotions. It is of course possible to study these thoughts about emotions, to determine their logical relations to each other, and to arrange them in a systematic scheme; and this is necessary to the science of æsthetics; but such treatment of the emotions bears an even looser relation to the psychology of the emotions than the syllogism bears to the psychology of inference.

Æsthetic pleasure is never pleasure in beauty in general, or in any general characteristic possessed by all the parts of the

[1] For the opposite view see Ribot, 'L'Abstraction des Emotions.' *L'Année Psychologique*, Vol. III., p. 1. Also Urban, 'The Problem of a 'Logic of the Emotions' and Affective Memory,' PSYCH. REV., Vol. VIII., p. 262 ff. and p. 360 ff.

[2] Cf. Calkins, 'Introduction to Psychology,' 1901, p. 264. Also the writer's review of Ribot's 'L'Abstraction des Emotions,' PSYCH. REV., Vol. V., p. 78 f.

object and noted before the beauty is felt: the emotion of beauty does not depend upon the assimilation of an object to its type. It is an immediate experience, the feeling of a relation between the ego and some sense-object. To enjoy the beautiful is to feel that self and something whose apprehension depends upon sense-processes, together, constitute one whole, to feel the same state of activity in a manifold of experiences, and to feel this reflectively, that is, discriminatingly, self-consciously. The æsthetic worth of an object is measured by the manifoldness of the details, each possessing its own emotional tendency, which unite in enhancing a single dominant mood; but it is also measured by the degree of inwardness and closeness felt to exist between the self and its object.

We are not advocating a mystical theory of the identification of self with things, or of thought with its object — that is a different matter. One feels in harmony with those stimuli which give him pleasure: one loses or forgets himself, is 'taken out of himself'; or takes the object over into himself, whenever he recalls or perceives them. Æsthetic judgment is like all judgment in this respect: it differs from many others in two ways, first, in being the enjoyment of something that stimulates sensation or the memory of sensation, and secondly, in that it occurs only to reflective consciousness, to a being or beings conscious of themselves. To be an experience of reflective consciousness means to be thought of as sharable by the entire class to which the self belongs, and this social reference is essential to the æsthetic judgment. Moreover, for the same reason, the beautiful object must possess some sort of human significance: in some way it must suggest the experience of man. Children enjoy bright colors, rhythmic series of sound or movement, and things that stimulate the sense organs and muscles to healthy activity, but not beauty, in the reflective sense of the word. One of the clues which lead children to self-consciousness and reflective experience is just the imitative effort to understand why older persons place such high values upon Sistine Madonnas, Beethoven Symphonies, St. Chapelles, etc. Perhaps the purest of æsthetic pleasures are those of classic orchestration, and they are so because, psychologically

speaking, self and sound, for the time, enter into one larger and more real experience — not really, but in play, in simulation and pretense.

In every great work of art, however, there are images suggesting opposed emotional tendencies or capable of suggesting them, and it may be asked how, if there is no emotional subsumption, such divergent tendencies can be joined within a single poem or other work of art. It is said that every work of art unites many emotional tendencies in a dominant mood, and the remark conveys some truth, but it is a mistake to say they are united as species are united in a genus. In Tennyson's 'Lotos-Eaters,' for example, the dominant mood of self-indulgent ease in a land where it seems always afternoon is apparently disturbed by the emotional tendencies of such images as,

> "wasted lands,
> Blight and famine, plague and earthquake, roaring deeps and fiery sands,
> Clanging fights, and flaming towns, and sinking ships and praying hands."

At first thought it may seem as though the dominant mood of this poem should be disturbed by the emotional tendencies of such images. As a matter of fact, however, one is not conscious, in reading the poem, of such disturbance; and if we were conscious of it, it would argue a defect in the poem itself. Nor are there reasons for saying that these images awaken unconscious emotional tendencies, related to the dominant mood as species to a genus and contributing something to the strength and richness of the former. That they strengthen the dominant mood and exalt the artistic value of the poem, everyone realizes. So also do the passages referring to home and country, with the calls to activity in their behalf which the memory of them brings with it — the passages beginning with, "Dear is the memory of our wedded lives, etc." But if these images do not disturb the mood of the poem, opposed as they are to it, I do not see how any theory of emotional subsumption can account for the artistic strength of the poem.

That the principle of contrast plays a large part in all art, and a very large part in the poetry of Tennyson, is well known. Almost any passage in Tennyson illustrates the fact — the

quatrain from 'In Memoriam' beginning, "Old Yew, that graspest at the stones, that name the underlying dead," for example. For two reasons such contrasts do not destroy the emotional unity of the poem. In the first place, it is impossible, as we have above seen, for the reader to experience two emotions at the same instant, and some sort of emotional unity is therefore certain to result from any reflective combination of sensuous images such as we have here. In order that the work be a work of art, it is necessary that the resulting emotion be not one of disgust, distress, or unsatisfied conation of the ideal kind; and in a mood-poem the dominant mood must be sustained. In the second place, the emotional unity is not disturbed in the 'Lotos Eaters' (*e. g.*), because the contrast-images are so handled as to define and emphasize the images of the gods lying beside their nectar, without awakening the corresponding contrast-emotions at all. If it be asked how contrast-images can be introduced without the corresponding contrast-emotions, we answer that feelings are usually slower than ideas to develop (as we have above seen), and that the contrast-images here follow each other in such rapid succession as to produce only a confused and vague result, like 'a tale of little meaning, though the words are strong.' What would be the result upon this poem if, instead of this rapid sequence, some one of the images like 'flaming towns' were dwelt upon at such length as to bring out all the agonizing details of a burning city? The poet relies upon the slowness with which emotions develop as compared with the contrast effect of the images themselves. Contrasted images tend to define and clarify each other, but contrasted or opposed emotions tend to destroy each other. It is now known that 'emotional expression' has much to do in determining the emotion itself, and of course the same muscle cannot both contract and relax at the same time, cannot 'express' both sorrow and joy, distress and comfort, at the same instant. To awaken emotions opposed to the dominant mood of a mood-poem would be to lessen or destroy the artistic value of the poem, but to intensify and define the imagery of the dominant mood by contrasts so used as to avoid awakening opposed emotions increases the art-value of the poem.

The self-reference of the so-called moral emotions is recognized by all. The simple liking of childhood develops into the gratitude of reflection with a marked egoistic self-reference at every stage of its history. So also with dislike and terror, hate and contempt. In sympathy, pity and mercy, an altruistic self-reference is involved, a reference to what Baldwin calls 'the bipolar self' in its unity of manifold selves. The consciousness of the identity of the private self with other selves, the sense of companionship and coöperation, is one of the profoundest joys of life, and it gives to public opinion all of its vast power to influence the conduct and the thoughts of men. Public spirit, that highest of the political virtues, is based upon it and involves it. Duty, obligation, self-approval, self-condemnation, remorse, and many other of the emotions and judgments of value whose objects are the private self and its conduct all involve feelings of harmony or discord between the private and the social self, or between one social self and another or others. These emotions are by all recognized as attributive elements in self-consciousness, which arise in experiences of particular persons and acts. There is no such thing as an emotion of duty in general, or of philanthropy toward mankind in general. What one commends, for example, is not charity, but charities; charity is the name for a particular social attitude or sentiment with reference to which one feels generality, and with reference to which one passes the judgment, 'charity is good.' The emotion which we experience with reference to charity in general is identical with the emotion present in all instances of conception, viz., an emotion or feeling of generality.

In the emotions of moral experience two closely related elements, a social and a private one, are present. A sense of personal worthiness or unworthiness to occupy one's place among his fellows, accompanied by an inner sense of discord, division and failure, or their opposites, as if I had injured or benefited myself by my own hand. One destroys his oneness with society and at the same time his oneness with himself, in his thought about himself, whenever he places himself under the ban of his own moral censure. The former, the sense of hagrin and defeat in the presence of one's fellows, is what is

meant by the loss of self-respect; the latter, the sense of disparity between one's actual self and that consistent and permanent self of one's faith and hope and love, is what we name conscience. The differentiæ of both kinds of feeling disappear as soon as we abstract the self-reference which they contain. Moreover, they are inseparable, and at times indistinguishable. The self of reflective consciousness is always a one-of-many, and the so-called unity of reflective consciousness is always a unity of one with many, a unity which the self has realized for itself by its own activities. The social consciousness is, in this sense, and in its form, bipolar, and whichever quality of moral experience one is conscious of, the cognitive content of self-consciousness is the same — only the point of emphasis, the focal point of attention differs.

Finally, the emotions of intellectual experience, emotions of wholeness or integrity, of generality, of necessity, and of particularity. In order that a society exist at all, there must be some medium of intercourse — gesture, cry, mimetic movement, or other. The consciousness of membership in any social group, whether immediate or reflective, will therefore be a consciousness of self (private or social) as a being who makes certain cries or other movements for the communication of thought; and, objectively, universality of significance among the members of the social group must attach to each gesture and cry before it can serve as a symbol for communicating thought and feeling. Here are the points of departure for the intellectual emotions of necessity and generality. Necessity, or the consciousness that the significance possessed by a certain symbol or series of symbols is demanded by the nature of the self of which we are conscious and by all selves of the same society, may be otherwise stated as a recognition of the necessity to society of attaching just that significance to that symbol. The 'theoretic ought' is rooted in this consciousness of social necessity. Universality, or the feeling-consciousness that what is true for one must be true for all selves, and the psychological dependence of my own conviction upon the consciousness that what seems true for me here and now is true, or will be found true, for all intelligent beings everywhere and always, is like-

wise a function of the social consciousness and inseparable from it. Integrity or wholeness in the objects of thought, likewise, is based upon the experience that other selves — that all selves, under the same conditions, must face the same object that I now face. Why should not the rocking-chair before me, like Faust's poodle, swell and contract, change into a hunch-backed beggar, or into a society swell all togged out with silk hat and cane? Why do children find no 'impossibility' in such stories as those of President Jordan to his children, about children who take off their legs at night with their clothes, the legs spending the hours when their owners are asleep in scampering out of the windows and in meeting all kinds of childish adventure? Why, if not because the child has only an implicit self-awareness, no consciousness of self as one of many, or as a unit whose very being is inseparable from certain forms of action and certain attributes of mind and body?

As the reflective social consciousness grows, the object, from being (1) a 'mere' object or external, becomes (2) a relatively permanent group of empirical properties common to the experiences of all selves with powers like mine, and (3) a 'substratum' of these properties, 'the permanent possibility of sensation,' a *Ding-an-sich*, independent of all private and particular selves. Hegel distinguishes these three uses of the word object, but he does not see that they are all functions of the social or self-consciousness, and that they appear in the individual's cognitive experience as he develops in reflection. The particular and the individual sustain just this genetic relation to each other: they are functions of successive stages of growth in the social consciousness, both being forms of what for want of a better term we may call the felt integral.

IX.

By way of recapitulation we may say, then, that thought and feeling cannot be separated or contrasted without destroying the reality of both. Thoughts are always shared experiences, while feelings are private and unshared; thoughts are always universal and, in reference, objective, while feelings are always particular and, in reference, subjective. Feeling is to

be regarded as an attributive element in self-consciousness, although self is not always explicit in consciousness and feelings are often read into the object and regarded as attributive there. In feeling, in other words, we experience immediately the relation of the ego to its object, a relation of unity or diversity which the ego itself establishes. The ego of which man is conscious cannot be a mere 'unconscious feeling,' or merely the state of the body, or the soul considered as a substance, but an activity whose product is a world of related selves and their experiences. As to the content of self-consciousness, we distinguished between immediate self-awareness and reflective self-consciousness. Self-consciousness in the first sense includes the empirical qualities of the body itself, together with a sense of externality to everything else within the range of perception or memory. Reflective self-consciousness is based upon the recognition that the self belongs in classes with other selves, that it is in a sense one with them, and that its experiences, therefore, possess a significance for them, and theirs for it. All feelings acquire a social reference, a universality of significance, from reflection, and thereby are transformed into ideal emotions which underlie æsthetics, ethics, the sciences of religion, and logic. The relational emotions cannot, however, be regarded as abstracts or genera to which other feelings are related as species. They are simply reflective, relational feelings which are immediately connected with the activities of the ego, particular feelings of particular activities. Relations obtain between the reflective functions of consciousness such that we may properly speak of the emotions of the logical processes and of volition, but I for one fail to find introspective evidence for the theory that there is an emotional logic of the emotions. I find nothing which seems adequately characterized as an emotional classification of the emotions.

DISCUSSION AND REPORTS.

DR. BOSANQUET ON 'IMITATION.'

Dr. Bosanquet's article in the July Review, which summer work in California has prevented my considering for the September issue, is in my view a very valuable statement of the conditions of the problem before him and me, and a clear account of his own views. I find it, indeed, so able and fair that it would seem that our discussion should, before we finish, clear up some aspects of the problem of social organization. I endeavor in what follows to take up the threads as he used them, and to put into the garment a further stitch or two.

I find it well to preface the discussion proper with a personal explanation. It was not as a late or 'after' thought that I added—'at last,' as Dr. Bosanquet says—the theory of 'selective thinking' to my general explanation of social organization by the development of the 'imitative thought-situation.' On the contrary, the former topic seemed to me from the first an essential one; and the solution reached was printed in the *Social Interpretations*, first edition (Chap. III., on 'Invention,' § 3, 'Selective Thinking'). While the positions were explicitly taken there as now by me, the treatment was so brief that I chose the topic for expansion in the 'President's Address.'[1]

Considering some such theory, therefore, as Dr. Bosanquet also does, as essential to a genetic account of the psychological thought-situation involved in social life, I shall direct my remarks mainly to the last part of his paper, rather than to the early portion in which he criticises the 'imitation' view apparently without taking the theory of selective thinking into account. Yet, before proceeding to what I believe to be the really essential differences between Dr. Bosanquet's view and my own, certain remarks of his made in the earlier connection deserve notice.

First, I fully admit the requirement that a genetic theory should account for—so far as genetic theories 'account for' anything—what Dr. Bosanquet calls the logical or systematic character of thought, both in general and also in this particular case. I maintain the theory of selective thinking as such a genetic account. Indeed, it will be

[1] This address is now incorporated *in extenso* in the new volume, 'Development and Evolution,' Chap. XVII.

seen below that I find Dr. Bosanquet's failure to press such a genetic theory to its ultimate issue the main defect of his view. Accordingly, I am in no sense denying the 'logical' point of view, nor ignoring the facts of which it takes cognizance, in my attempt to supply a genetic theory. Here I think Dr. Bosanquet's account of my view, as based upon or requiring only 'repetition,' is liable to mislead. From the standpoint of scientific observation, and possibly also from that of analysis of content, imitations may be described as 'repetitions'; but that does not mean that repetition is a sufficient statement of the result in which the imitative functions issue. In the second part of this discussion I shall raise the general question as to the relation of imitative or repetitive mental contents to the form of 'logical' organization which involves them.[1]

The dispute as to the rival claims of 'resemblance,' on the one hand, and 'identity in difference,' on the other, seems to me so verbal and formal that time is not profitably employed in discussing it. It was for this reason that I failed to find a point of essential criticism in Dr. Bosanquet's former argument, where it was this distinction which was principally urged. Whether in biology we say, 'here are two individuals which resemble each other in this or that respect,' or 'here are two cases of the same identical character showing so much variation or difference,' is of little moment. The important matter is what use the scientist makes of the phenomenon thus alternatively described. I see the importance of the distinction from the point of view of a philosophy of individuality, and also from that of the logic of distinction and classification; but for empirical research I think the gain accrues from the emphasizing of differences, even to the extent of naming differently things which in some ways may show resemblance—as in the distinction between imitation and invention (to which Dr. Bosanquet objects) in current sociology. In my own theory, invention arises in imitations and is born of them, and there is a large mass of identical process in the two; but still, to the observer in sociology, the inventor is in type of performance, and in its effects upon the social body, as different from the typical imitator as a constructive thinker is from a parrot. Yet it may well be that I fail to catch Dr. Bosanquet's full meaning on this point.[2]

After these preliminaries I go at once into what I consider the

[1] The second part is to appear in the January issue. A request from the Carnegie Institution for advice in matters psychological delays the further writing.

[2] I confess I am not yet familiar with his biological criticisms made from this point of view.

essentials. Dr. Bosanquet makes two points—one an admission and the other a criticism—which sharpen the issue well; and what I have to say may be put under these two topics—in the reverse order of his argument. The first point is that as to the need and the extent of possible genetic analysis in this case; the second is that as to the adequacy of the imitative solution in the form of the 'self-thought' and 'selective thinking' theories.

I. In the first edition of my book I pointed out that thinkers of a certain class, whom I called 'idealists'—as holding to an 'ideal theory of social life'—'make the assumption of publicity.' "What is wanting," I go on to say ('Social Interp.,' 1st ed., 503), "is the bridge from the private thought to the public thought. * * * Given complex social situations, whence their validity for all the members of society equally, and whence the intrinsic element of public reference which is a necessity of social nature to us all? * * * They fail to describe the process or type of function by which the social matter becomes [or better, *is*] public, and is so made available for society and for the individual both at once" (pp. 504, 505). In my view it is the imitative process of assimilation and growth which supplies this requirement—the requirement that we depict what actually occurs in the genetic progress of consciousness in thinking a 'public' thought.

Now it is extremely interesting to me that Dr. Bosanquet explicitly admits that he makes this assumption. Speaking of the individual's 'grasp of the relation between the persons involved,' he says, "I do not see the initial difficulty of obtaining such a grasp, which is a condition of the general will or self. It is implied in mental process from the first, through the principle that contents operate as universals, assuming differences in accordance with the details through which they are reproduced. As regards the participation of different intelligent individuals in one such grasp, the true principle seems to me to be that an idea normally operates throughout several intelligences just as within one. * * * I mean that, in all cases the ultimate nerve of intercourse is that according to a nature common to all concerned, one thing follows from another. * * * The thing so following, whether in myself or in another, I equally recognize as the continuation and completion of my thought. * * * The nerve of the whole process is that, given the data, including their own resources, other minds bring out, in a form prescribed by their powers, the conclusion at which mine is aiming. The point is, that it is a *conclusion*"—communication depends ultimately on logic (PSYCHOL. REVIEW, July, 1902, pp. 385, 386).

Here the issue is clearly joined: shall we assume, at a stroke, so-

cial organization through a number of minds acting—thinking—alike on the same material, or shall we ask by what type of actual social experience they accomplish this? The latter inquiry, as I conduct it, simply aims to discover the normal vehicle of what Dr. Bosanquet properly calls the 'normal' operation of the mind. Suppose we admit the 'principle that contents operate as universals'—a phrase itself obscure enough, indeed—still there must be concrete images, objects, thoughts, processes, which body forth, as it were, the 'universals' and show the 'operation.'[1] I suppose that Dr. Bosanquet would hold that there is some mental function—say sensation—and some 'logical' process (in his sense)—say my castles in Spain, or my solution of the coal-strike problem—which I do not hold others to; constructions which are tentative, personal to me, not of social value, or *not yet* so. Now I ask what further function, psychic process, does give social value to some 'logical' constructions and mark their public character. I hold that whatever is a process, whatever 'operates,' whatever we can attribute to consciousness, *must show itself in some sort of empirical movement.*

The same sort of claim in kind, as that of Dr. Bosanquet here, is made—to cite other instances in current discussion—by those who say that the 'self' may be assumed with no sort of scientifically observed empirical process of growth and determination in actual consciousness; and Professor Ward seems to hold that while 'mental activity' is discoverable, still no particular type of changes of content can be pointed out as revealing it. This sort of claim is, in all these instances, I think, sheer mysticism. Whatever is, *shows.*[2]

Now, in fact, in my own answer to the question I reach a process which would seem to be general[3] enough to please anybody—that of the rise of self-consciousness by functions which normally implicate other selves; and I hold that there is here the normal operation of the processes involved in the 'grasp' which Dr. Bosanquet speaks of.

But this is not the only point at which Dr. Bosanquet makes, as he himself allows, an assumption—one which sets limits to the genetic point of view. He assumes the so-called 'logical process,' or

[1] Unless we postulate a real general or universal self or will whose nature it is just to 'come to itself'—as I say in my book—in individuals in essential independence of the actual experiences of their mental lives.

[2] The question of mental constitution or endowment is, of course, a legitimate one, and certain 'characters' of mind, such as 'laws of thought,' etc., may be taken up from the point of view of racial evolution (see the second part of this discussion in the January issue).

[3] I avoid the term 'universal'—only trained philosophers are 'sure' in their meaning of it!

the 'organization of thoughts' and their progressive determination. He says, "selective thinking is an improper phrase, because thinking as such is selective and more than selective, being selective, so to speak, by construction of determinate variations. Professor Baldwin has a leaning toward determinate variations. I believe that this is the crux of the whole matter, and that if the origin of determinate variations be fairly considered the idea of selective thinking must go" (*loc. cit.*, p. 388). This is simply and frankly to say that no possible tracing of the genetic process of organization of thoughts in 'logical,' or better 'systematic,' wholes is possible. Now my reply repeats the point just made above. I say, suppose it be true that all thinking is selective, still genetic science has to ask how selection works. Why are some thoughts[1] selected, while others are not. What function is *par excellence that in which selection occurs?* Why one particular line of determination at one time, at another time, another? Why is thought determined differentially, preferentially, polytypically? If one simply calls a halt and says, there is no answer to these questions, for the simple matter is that the mind, for no reasons and by no regular processes, makes its truth what it will, showing no process of tentative experience, of trial and error, etc., then well and good; then that is the end of discussion. But to me it is to go back to the essential mysticism of the *a priori* formalism which prevailed before the rise of the genetic point of view. The same question vexes the soul of the biologist: the assertion of 'determinate variations,' in the narrow sense of the claim that no natural reasons can be given for whatever balance of variations may be found in a given direction, and consequently that since science cannot account for them they are part of the inner character—the mystery—of life. This, I believe, in biology as in psychology, is a resort to what is called above sheer mysticism.[2]

[1] Broadly defined, as 'cognitive wholes,' or 'mental objects' (cf. Stout's definition of 'object' in my *Dict. of Philos. and Psychol.*, Vol. II.).

[2] Dr. Bosanquet cites a passage in which I say that new inventions are always variations from functions already attained—variations from what, in the address on 'Selective Thinking,' I called a 'platform.' This is very different from saying that the variations are themselves determinate, that is, that more than the probable number lie in any particular direction. Determinate variations in biology would imply an uneven distribution of cases about a mean, apart altogether from the relative position of the platform from which the whole series of them is projected. I have pointed out, in *Development and Evolution*, p. 160 ff., the common confusion that prevails on this point. Any scheme of thought variations is on a platform; but my personal opinion, based on the exact statistical researches now available, is that they are themselves indeterminate, and that just for this reason a process of selection is necessary as in biology.

I think with Dr. Bosanquet that this is the 'crux' of the whole matter: is there some intrinsic principle of determination which does not show itself in any recognizable conscious process of growth? If so, then no genetic account of it can be given. I believe that both here and in biology nature works by phenomenal changes, and by constant laws, and that nowhere, at no point in the backward regression of events, can science stop and say, beyond this there is a something, it is true, which issues in determinate phenomenal organization of contents, but which is itself not observable as change in content. So if Dr. Bosanquet really does *give up the task of showing* how selective thinking works, and make it simply an assumption, then I don't see how the 'crux' between him and me is to be overcome. It follows, too, and of course, that if no account can be given of the selective character of thought, then the account of it in terms of imitation and selection—my account of it—is ruled out. And I count it one of the services of his article to have stated this issue—in terms to which we both subscribe—for the interested reader. The remarks on the origin and meaning of 'truth,' in my chapter on 'Selective Thinking,' join the issue in about the same way.

II. But seeing that Dr. Bosanquet does discuss the theory and does bring certain specific objections to it—besides the general one that any such genetic account is impossible—I shall reply, as far as I can, to his criticisms. In the first place, it will be seen from what is said above, that I do not allow that the alternatives read *imitation plus selection*, OR *organization* (his 'logic'); but, admitting the organization, I hold that it is a question of *organization by a process of imitation plus selection*, OR *by some other genetic conscious process.* I am not able to stop anywhere and say, here no further genetic process can be traced and so much organization as here exists must be flatly assumed.[1] The scientific spirit of enquiry may be at points permanently baffled, but it does not admit miracles!

So, claiming the legitimacy of some genetic account of the process of organization—what I have called 'systematic determination'[2]—the

[1] In remarking that Dr. Stout seemed to me to be tending toward a theory of selection, I did not mean that he was in any sense giving up his doctrine of thinking by organization in a logical 'plan of the whole.' I myself go a great way with Dr. Stout and with Dr. Bosanquet in this doctrine; but I meant that Dr. Stout seemed to admit the legitimacy of finding a genetic account of the method of growth of such logical or teleological wholes. My impression is based largely on personal conferences with Dr. Stout in connection with our work for the *Dictionary of Philosophy and Psychology.*

[2] *Development and Evolution*, chap. XVII.

narrower question is: can we say that the imitative self-thought theory combined with the selective thinking theory is a valid account of the case of organization which we describe as social thinking?

Dr. Bosanquet seems to admit—what I think a careful reading of his discussion will bear out—that much of his criticism of the social imitation theory would be met in case the theory of selective thinking were made out. I shall accordingly take up in the next issue, if possible, his objections to this latter theory.

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METHODS OF TESTING RELATIVE PITCH.

A comparison of the methods that have been used for testing perception of pitch is of importance, as the results that have been obtained vary considerably.

In the *Yale Psychological Laboratory Studies*, 1893, I., p. 80, Gilbert reports experiments on the 'Musical Sensitiveness of School Children.' These experiments were confined to testing perception of pitch differences. A pipe was used by which the experimenter produced tones separated by $\frac{1}{32}$ of a tone, or multiples of that amount. The apparatus is described fully in his paper. The method of gradation was used. Starting from notes a considerable distance apart, the experimenter made the difference less until the subject declared that he perceived them as the same. The process was reversed and the average of the two results taken. An average of ten such experiments with each child was taken. The results indicated that children of six years could, on the average, discriminate tones $\frac{12}{32}$ of a tone apart, and that the improvement was fairly uniform to the age of 19, at which age a difference of $\frac{3}{32}$ of a tone was, on the average, perceived.

In employing the method of gradation with a string instrument I encountered the serious objection that a hypercritical attitude, either constitutional or suggested, on the part of the subject works constantly for a better, the opposite attitude for a worse record. That is, a person who fancies he notes differences where there are none perceptible by him, or confuses quality or intensity differences with pitch difference, secures a better average than one who records more accurately what his senses give him. By the method of right and wrong cases this difficulty may be avoided. Differences of quality and intensity are very likely to arise in any pipe instrument, especially when the mouth is used. Moreover, as by this method the difference in pitch con-

tinually varies, the subject is poorly provided with a standard difference with which he may compare, and from which he might distinguish qualitative differences. From Table II. it may be seen that this is important. The best subjects show their superiority in perceiving notes of the same pitch as the same. The poorer subjects tend to call all notes different, confusing qualitative with pitch differences. A further objection to this method is its length.

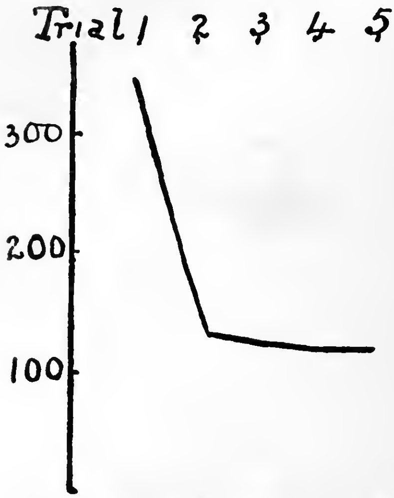

FIG. 1. Percentage of Mean Deviation of Successive Trials.

In testing freshmen and seniors for perception of pitch, the Psychological Laboratory of Columbia University uses a wire, 1 meter long between supports, with a movable bridge. A note is given with the bridge at 750 mm. from one end (giving F_1), the bridge is placed at the center, and the subject tries to reproduce the tone given. Then the original tone is again given, the bridge is placed where the subject left it, and he tries again. The average of the two results is taken. The weakness of this experiment is that the tone memory is tested as well as the tone perception; this is only partly remedied by replacing the bridge for the second trial where the subject had left it at the first trial. It is not claimed that the results are more than approximate and relative. Of those tried, 10 per cent. have an error of less than .1 of a tone; 53 per cent. .1 to 1 tone, and 37 per cent. more than one tone. These errors are far greater than those of school children tested by Gilbert and by me.

In examining the Columbia Laboratory method I prolonged it to ten trials, always replacing the bridge for each trial where the subject had left it at the preceding trial. I made fifteen such experiments. For each subject I computed the mean deviation, brought it to a fraction of the average error of that subject, to make it comparable with other cases, and took an average of the resulting fractions. The average deviation was .706 of the average error, which shows a large element of chance. In a similar manner I found what fraction the average deviation of the first, second, third, fourth and fifth trials were of the average deviation of the last nine trials in each experi-

ment (see Fig. 1). It is clear that the first trial is not comparable with the others. As I also found that the first trial gave an error far greater than the remaining trials, and the second trial one considerably greater, the average of these two trials indicates a perception of pitch far worse than that actually possessed. However great a number of trials is taken, this method is open to a further objection. A subject may by chance get within his power of discrimination and may remain there, for there is no apparent reason why he should move the bridge further away from the correct place (see Fig. 2). All the records on this figure were made by the same subject.

For these reasons neither Gilbert's method nor that in use at Columbia seem very satisfactory for anything like an absolute determination of pitch perception. To secure necessary thoroughness without too great expenditure of time we must look, I think, to methods suitable to use with classes. An individual test, however, is sometimes valuable to confirm the class test. The method of 'right and wrong cases,' in which the operator produces the tones, is too long. The subject must reproduce the tone therefore. This may be done by the voice: but in the absence of any practical phonautograph an accurate test of pitch perception cannot be obtained in this way. All wind instruments, it seems to me, interfere with the memory of sound because of the continued and varying note given during adjustment. Those that require the use of the mouth interfere with the subject's attention, and have other faults.

As an improvement on the Columbia method above described I suggest the following: Use two strings, 1 meter long, the tension being adjusted so that the note given by one is equivalent to that given by the other when the bridge is placed, say, 750 mm. from one end. Within an octave of middle C, any note that can be readily checked with a tuning fork will do. The bridge should be just as high as the wire. The movable bridge should slide in a groove, and should clamp the wires with wooden surfaces pressed together by a spring. The subject should face the instrument and handle the movable bridge until he can move it without special attention to it, and until he understands what effect on pitch motion in either direction has. The purpose of using two strings and this kind of bridge is to save time, confusion and distraction of attention, all of which introduce the question of tone memory into the experiment, which is, of course, an entirely different matter from tone perception.

The bridge is put at the center of the one string and the standard note is given on the other. This the subject tries to reproduce. When he stops, the standard note is given again, and the subject tries

again. The result of the second try is recorded for averaging. For a second experiment the standard note may be changed by placing the bridge at, say, 800 mm. from one end of the standard string, which would be equivalent to 600 mm. on the string which the subject uses. The error at the second attempt is noted, corrected by being multiplied by $\frac{5}{4}$, and averaged with the first record. The average can

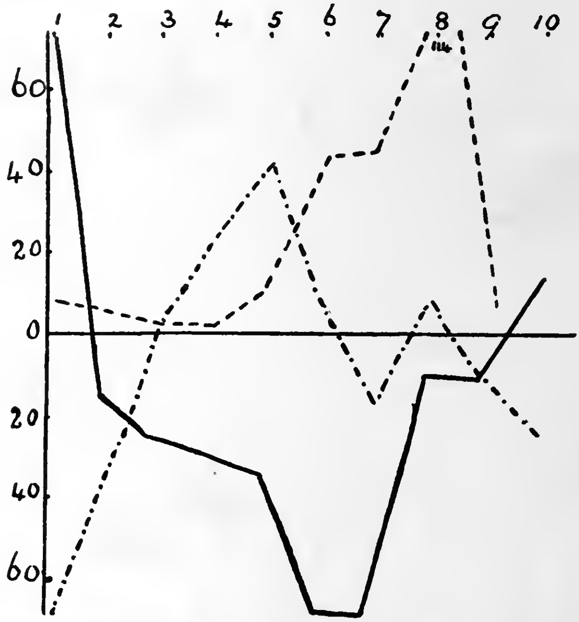

Fig. 2. Three records made by a graduate student according to Columbia method. The ordinates show the errors in mm.

easily be reduced to fractions of a tone. These two tests can readily be carried out in five minutes with the average scholar.

Working with a rather poor instrument I found that, in 72 trials, the mean deviation was thus reduced to .48 of the average error, as contrasted with .706 by the present Columbia method.

I think that methods suited for use with a class are better. I first used tuning forks that gave respectively 512 and 516 double vibrations per second. By adding wax I could get intervals of $\frac{1}{6}$, $\frac{1}{14}$ and $\frac{1}{28}$ of a tone, without causing thereby any marked differences in quality between the forks owing to the mass of wax on either of them. To find what interval people capable of musical training possess, I secured the coöperation of a 'mixed' church choir, and of the ladies of two of Professor Farnsworth's classes at Teachers College. Pro-

fessor Farnsworth and the choirmaster kindly graded the subjects into classes 'a,' 'b,' and 'c,' according to musical ability.

I first required the subjects to say which of two notes given was higher. The results are given in Table I. They show that musical

TABLE I.

Interval	$\frac{1}{6}$	$\frac{1}{14}$	$\frac{1}{28}$ tone.			
No. of trials	10	20	10			
Class *a*	95.7	63.5	58 per cent. right.			14 subjects.
" *b*	69	48	37	"	"	9 "
" *c*	50	65	54	"	"	8 "
Unclassified	70	55	49	"	"	8 "

ability corresponds rather closely to capacity to perceive pitch differences. Further, that by this method an interval of $\frac{1}{6}$ tone is probably a little too small for eliminating those that are incapable of musical training. Some of the better musicians showed a tendency to call the lower notes the higher, and conversely, at every trial, a fact that obviously discredits this method.

I then required the same classes to tell whether the tones given on the forks were the same or different. Table II. shows the results.

TABLE II.

Interval		$\frac{1}{6}$			$\frac{1}{14}$			$\frac{1}{28}$ tone.	
No. of trials		20			30			20	
	'Same.'	'Different.'	Total.	'Same.'	'Different.'	Total.	'Same.'	'Different.'	Total.
Class *a*	80	85	83.3	67.7	61.7	64.7	75	61.7	70.7
" *b*	71.4	90	83	61	72.4	68	61.6	70	66
" *c*	66	96	81.5	68	81.3	74.7	56	56	56
Unclassified	33.3	100	66.6	50	63.3	56.7	13.3	66.3	40

The numbers indicate the per cent. of answers right.

The words 'same,' 'different,' there indicate that the per cent. given below is the number of right answers given when the tones struck were the same or different. An equal number of each was given in the experiments. It is clear from these figures that when the subject begins to get uncertain, his difficulty is usually not so much in perceiving that the tones are different when they are different, as in telling that the same notes have been struck, *i. e.*, in distinguishing qualitative from pitch differences, or, perhaps, in checking his imagination.

In the use of the last method I judged that $\frac{1}{6}$ tone is too small a difference to select the best ears, too large to indicate the really deficient ones. I therefore selected $\frac{1}{3}$ tone and $\frac{1}{9}$ tone as the intervals for

a series of tests with school children. As I could not get suitable tuning forks I strung two wires so as to give the same note for their full length, namely middle C, and, by means of a bridge, secured the above intervals. I gave twenty trials at each interval, in alternate series of ten each, making the number of 'sames' and 'differents' equal. Tables III. and IV. and Fig. 3 show the results. I cannot

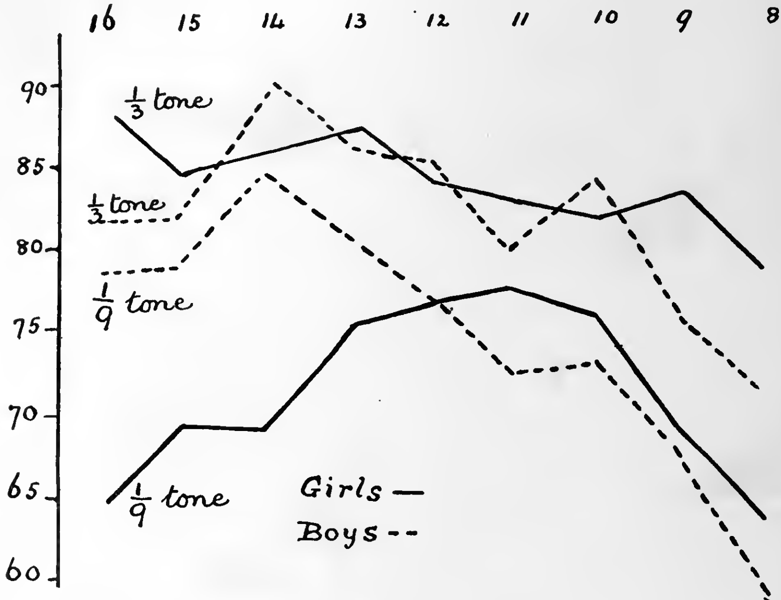

FIG. 3. Percentage of Right Answers.

TABLE III.

PERCENTAGE OF SUBJECTS FAILING TO GET MORE THAN 75 PER CENT. RIGHT.

Age in years	16	15	14	13	12	11	10	9	8
Number of subjects	5	17	28	41	27	30	29	33	17
Girls, $\frac{1}{3}$ tone	0	23	18	15	19	33	34	34	35
Girls, $\frac{1}{9}$ tone	100	76	68	66	52	50	58	90	95
Age in years	16	15	14	13	12	11	10	9	8
Number of subjects	5	10	28	41	27	30	29	33	17
Boys, $\frac{1}{3}$ tone	40	30	7	20	23	35	29	50	60
Boys, $\frac{1}{9}$ tone	20	50	25	35	48	58	50	80	80

TABLE IV.

PERCENTAGE OF CORRECT ANSWERS.

Age	16	15	14	13	12	11	10	9	8
$\frac{1}{3}$ tone	86	84	88	88	85	82	84	78	75
$\frac{1}{9}$ tone	72	73.5	77.5	78.7	77	75	75	68	59

explain the poorer showing of the older girls with the small interval as shown by the curve. Younger girls did better at the same time under the same circumstances.

I examined the papers, taken at random, of thirty-five boys and girls, and found the same tendency as in the former experiment to err chiefly in telling which notes were the same. Out of 308 errors in 1,400 answers: 178, or 57.8 %, were 'same' called different; 130, or 42.2 %, were 'different' called same; in conducting the experiment the instrument was kept from view, one or more teachers watched with me for any signs of copying or prompting, and, especially, I was careful to get the interest of the children in securing correct results. The first note must be dampened before the second is sounded, so as to prevent beats. Great care must be used to strike the string at the same distance from the ends and in just the same way.

The fact that the subjects discriminated an interval of one third tone not much better than one of one ninth tone may, perhaps, be taken to indicate that the growth in accuracy is a matter of cerebral more than of aural development. Another indication in this direction is that the older children in a class, who are more likely to be 'left backs' did rather worse than the rest of the class, as a rule.

Two or three of the teachers remarked on the apparent connection between general intellectual inferiority and inferiority according to these tests.

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PSYCHOLOGICAL LITERATURE.

NEUROLOGY.

Die Abhängigkeit des Hirngewichtes von dem Körpergewicht und den geistigen Fähigkeiten. OTTO SNELL. Arch. f. Psychiatrie, Bd. XXIII., 1892, S. 436–446.

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Ueber die Abhängigkeit des Hirngewichtes von der Körpergrösse beim Menschen. EUGEN DUBOIS. Archiv f. Anthropologie, Bd. XXV., 1898.

On a Formula for Determining the Weight of the Central Nervous System of the Frog from the Weight and Length of the Entire Body. H. H. DONALDSON. Decennial Publications of the University of Chicago, Vol. X., 1902. Pp. 12.

On the Mitosis in Nerve Cells of the Cerebellar Cortex of Fœtal Cats. S. HATAI. Journal of Comparative Neurology, Vol. XI., No. 4, 1901.

On the Presence of the Centrosome in Certain Nerve Cells of the White Rat. S. HATAI. Journal of Comparative Neurology, Vol. XI., 1901.

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Number and Size of the Spinal Ganglion Cells and Dorsal Root Fibers in the White Rat at Different Ages. S. HATAI. Journal of Comparative Neurology, Vol. XII., No. 2, June, 1902.

Address of the President of the Anthropological Section of the British Association for the Advancement of Science. D. J. CUNNINGHAM. Science, N. S., Vol. XIV., Nos. 355, 356, Oct. 18 and 25, 1901.

In any animal which possesses a nervous system it should be possible to determine the amount of substance which has been differ-

entiated into nerve tissue and to compare this amount with that of any or all the tissues that form the remainder of the body. Owing to the mechanical difficulties which stand in the way of dissecting out the entire peripheral nervous system in any animal, and the spinal cord in the larger animals, it has come about that in vertebrates the encephalon has been, for the most part, the only portion of the nervous system measured with sufficient care to permit of comparisons. Of course the weight of the encephalon is by no means a constant fraction of the weight of the entire nervous system, nor is the proportion of the active nerve substance proper—that forming the neurones as contrasted with the medullary sheaths and the non-nervous portion of the encephalon—similar in the encephala of different species of vertebrates. Nevertheless, despite departures from uniformity and the limitations following a comparison of the encephala alone, a study of the weight relations of the encephalon in several classes of vertebrates leads to suggestive conclusions.

For more than a century, anatomists have been in search of an explanation for the observed fact that, even within the limits of a given order or genus of mammals, the weight of the encephalon did not increase in proportion to the weight of the entire animal, but was represented by a fraction which became progressively smaller as the body-weight of the species became greater. This rough statement of these relations was the best available when Snell published his observations in 1892. For his purpose he distinguishes two relations of the nervous system—represented by the encephalon—with which variations in weight might fairly be correlated. The first of these is the relation of the encephalon to all the metabolic activities of body—and the second, the relation of the encephalon to the psychical activities of the animal. In a general way, the larger the mass of the animal undergoing metabolic changes, the larger will be the amount of nerve tissue required, and also, within the limitations of class and order, the more complex the psychological activities of the animal the greater the mass of the encephalon.

Regarding these relations of the nervous system as factors in determining its mass, Snell designates the former as the 'somatic factor,' and the latter as the 'psychic factor.'

Beginning with the 'somatic factor,' he states the general proposition based on studies in metabolism, that the amount of metabolic change in animals of like form but different body-weight, is proportional to the extent of the body-surface, and not to the body-weight. In order to get this relation into a mathematical form, he defines

animals of like form as those having similar geometrical figures, the surfaces of which are therefore proportional to the squares of their respective diameters, and the volumes, to the cubes. It is further assumed that the specific gravities of the several encephala to be compared are so nearly alike that the relations between their weights are identical with the relations between their volumes. Hence it is possible to translate weight relations into volume relations directly.

Under these conditions, if it is desired to compare the development of the encephalon as represented by its weight in two animals of approximately like psychical abilities and similar form, but differing in body-weight, as, for example, a mouse and a rat, it would follow, according to Snell, that their brain weight would stand in the same relation as do the areas of their bodies. Thus if a represent the body-weight of the mouse and b that of the rat, and the weights as above stated stand in the same relations as the volumes of the bodies, then $\sqrt[3]{a}$, and $\sqrt[3]{b}$ will give comparable diameters of the bodies and the squares of these diameters expressed thus:

$$(\sqrt[3]{a})^2, (\sqrt[3]{b})^2$$

will be related as the areas of the bodies or as their surfaces. If h and h_1 represent the weights of the encephala for mouse and rat in the order named, then

$$h : h_1 :: (\sqrt[3]{a})^2 : (\sqrt[3]{b})^2$$

Taking the square of the cube root of the body-weight is equivalent to raising it to the 2/3 power or expressed decimally the 0.666 power.

The former may then be written: $h : h_1 :: a^{0.666} : b^{0.666}$. This exponent (0.666) is thought by Snell to be probably too small for mammals, and he proposes 0.68, which is slightly larger, as the better value for the 'somatic exponent.' In Snell's article, there is no evidence that he tested the validity of the conclusion here reached, but assuming that it was valid, he passed at once to a determination of the 'psychic factor,' or that difference in the weight of the encephalon which was to be correlated with the psychic development of the animal.

His procedure may be exemplified as follows: It is desired to compare the weight of the encephala of the mouse and of man. If the total weight in each case is the product of the somatic and psychic factors into the body weights, then b being the body-weight of mouse, and c that of man, and p and p_1, being the respective 'psychic factors,' it follows that the weight relations of the two encephala will be as $p.b^{0.68} : p_1 c^{0.68}$.

In any instance where the brain-weight is known, therefore, a division of the brain-weight by body-weight raised to the 0.68 power will give the number designated by Snell as the 'psychic factor.' If, now, animals for which the psychic factor has been found be arranged in a series determined by a diminishing value of the 'psychic factor,' it is found that the order in which the animals appear in this series is very close to the order in which they would be arranged were their psychical abilities determined by direct observation.

For example, by this method man leads the list with a psychic factor of 0.874 and a relative brain-weight (*i. e.*, the relation of the weight of the encephalon to that of the body) of 1/35, whereas the field mouse is down towards the end of the list with a psychic factor of only 0.045, though the relative brain is nearly the same, being 1/37.

In other words, due allowance being made for the relations of the encephalon to general metabolism, the psychical abilities of vertebrates are closely correlated with the weight of the encephalon.

This problem of the relation of the weight of the brain to the size of the body in mammals was taken up again by Eugen Dubois in 1898. Dubois discusses the differences in functional activity between animals of like form but differing in size, also the differences in the arrangement of the nervous system so far as they depend on size. He then proceeds to render Snell's formula more precise and to test thoroughly its applicability, using for the purpose Weber's records (1897) of the brain- and body-weights of mammals—the most reliable and complete catalogue thus far published. In taking up the subject Dubois starts with the propositions: First, that two animals of the same body-weight differ in brain-weight according to their position in the zoölogical scale; and second, that in the case of two animals having the same grade of brain organization, the animal having the greater body-weight will also have the greater brain weight. The weight of the encephalon depends therefore on (1) the degree of cephalization and (2) on the weight of the body. Dubois then makes a definite advance by pointing out the connection between the area of the surface of the body and the increase in the number of sensory or afferent nerve fibers by which this surface is innervated. If the arrangement of the nervous system is viewed further from the standpoint of segmentation, and each segment of the body considered as innervated by a sensori-motor mechanism, these segmental mechanisms being joined by the connecting tracts which constitute the bulk of the central nervous system, then, on comparing two animals of different body-weights but of the same grade of organization, it is impossible to

picture the differences as depending in the first instance on the development of the sensori-motor mechanisms for each segment, which increase in weight in proportion to the increase in the skin area to which they belong, and since the grade of the two animals compared is the same, the central tracts uniting the segmental mechanisms are also proportionately increased. Since the encephalon is in the main merely a complex of such uniting tracts, they would consequently increase in weight in proportion to the increase in the area of the body surface, and there would follow the increase in encephalic weight according to the hypothesis of Snell.

It is evident, however, that the analysis of the sensori-motor segmental mechanism can be carried much further.

In the first place it has been assumed that the constituent elements in the animals compared are alike in size, and that the larger area of the larger animal would be innervated by a proportionately larger number of fibers each having the same area in cross section as in the case of the smaller animal. Again, in the larger animal, it has been assumed that the increase in the other areas supplied by nerves is the same as that which occurs in the skin—but perhaps the point which it is most necessary to determine is whether the sensory innervation in the larger animal is of the same density as in the small. Taking the retina as a guide, Dubois argues that since the retina becomes relatively diminished in the larger animals, it is probable that the other sensory supply to the surface behaves in the same way, and that as a consequence, the sensory surface of the larger animals is less densely innervated than we should expect to find it by Snell's hypothesis; as the result of this, the connecting tracts in the central system would be relatively less numerous and of smaller weight and the encephalon of the larger animal therefore have a smaller weight than the theory demanded.

On the efferent side of the reflex mechanism, the relation between the increase in the weight of the muscles and the number and size of the nerves innervating them still offers a number of unsettled points. It appears probable that the increased nerve supply to the larger muscles depends rather on the relative increase in the area of the cross section of the muscles than on any other change.

However, the effects of alterations in this part of the arc are probably of least influence on the weight of the encephalon, and a much more important factor is to be found in physical features of the cerebral cortex and the formation of gyri in the encephalon. Dubois states that in the encephalon of mammals the cortex has nearly the same

thickness, no matter what the size of the animal. This is probably an over-statement of the small variability of this layer.

Nevertheless, it is true enough to direct attention to the source of a real difference between the brains of large and small animals having the same form, since the failure of the cortex in the larger animal to increase proportionately in thickness necessitates an extra increase in area, thus causing a folding and a formation of gyri, and a brain with gyri is smaller and weighs less than a brain composed of the same number of elements and equally complex would weigh were the cortex not folded.

The foregoing paragraphs serve to show the way in which the relations between the encephalon of the small and that of the large animal of the same zoölogical grade have been analyzed by Dubois. As the result of such an analysis, Dubois was led to anticipate that the weight of the encephalon would increase more slowly than the formula of Snell demanded. This Dubois tested by determining the 'somatic exponent,' or, as he prefers to name it, the 'exponent of relation,' for some seven pairs of mammals, selected from Weber's list. Here the weight of the body as well as the weight of the encephalon is given in each case, and the value of r, the 'exponent of relation,' is worked out. The seven values of r range between 0.5412 and 0.5854, the average of all seven being 0.5613. It will be seen that this is decidedly less than the value of 0.68 finally adopted by Snell. The smaller value found by Dubois is accounted for by considering that one or all of the factors which tend to make the difference in the weight of encephala relatively smaller than that in the area of the body, have been active in these cases. In one group, namely, the bats, where the change in size is not very great and is accompanied by the least modification in the enlargement of the encephalon, the increase in the weight of encephalon has been nearly proportional to the increase in the area of the body, and the value of r is 0.6649, which is practically Snell's theoretical value. It appears from this that the enlargement of the encephalon in the larger animal is certainly modified by the conditions which have been named, and probably it is further modified by conditions touching the finer structure of the encephalon — the data for which are not yet available.

Passing next to the 'psychic factor' of Snell, we find this represented by what Dubois calls his 'exponent of cephalization,' a term much to be preferred to that of Snell.

The 'exponent of cephalization' is represented by a number which, when multiplied by the weight of the animal raised to the

rth power, gives a product which is related to another product obtained by the same process, as are the respective encephalic weights of the two animals compared. Expressing this in a formula and using the same symbols which Dubois employs, we have:

$$E : e :: C.S^r : c.s^r$$

where E is the weight of the encephalon in the large, and e that in the small animal; S and s, the body weights, and C and c the 'factors of cephalization' for the same large and small animals, the weights of whose encephala are given.

It is thus possible to arrange animals in a series according to their 'cephalization exponents,' and the position in that series is found to correspond closely with the degree of psychical development exhibited by the living animal. There are limitations to the use of the 'factor of cephalization,' however, and it must always be remembered that in the first instance it is based on weight only, while there are ways in which the complexity of the encephalon and its physiological efficiency can be altered without necessarily causing any corresponding alteration in its mass.

By this investigation Dubois has made a most noteworthy contribution to our knowledge of the comparative anatomy of the nervous system. By correlating the skin area and the nerve supply to it, and then the development of the remainder of the system in relation to this afferent division supplying the skin, the changes in the mass of the central system are anatomically linked with those occurring in the rest of the body. It may be fairly said that now we can explain why the brain in large animals increases in weight so much less rapidly than does the body.

On the way to his general conclusion the author has entered a very large number of queries concerning possible differences which may exist between the brains of different animals, and the attempt to find answers to the questions should stimulate much important work. Of course, as the amount of metabolic activity varies with the area of the body surface, there is an indirect correlation between the metabolic activity and the weight of the central system, but it would appear more probable that the increase in the weight of the nervous system in larger animals was not dependent on the fact that the metabolic changes in them were greater and needed the increase in nerve tissue to control them, but because each new area of skin required to be innervated, and with the introduction of the new afferent elements other elements in the central and efferent systems were necessarily

added in order to complete the connections over which the additional impulses must pass. We thus come to view the increase in the mass of the central system as correlated with an increase in the number of afferent elements composing its sensory side, the increase of the other parts following in an orderly way its expansion at the periphery. Encouraged by this work on mammals, Dubois went forward to see whether an 'exponent of relation' could be determined when the brain-weights of men having different body-weights were compared. As the determination of the proper body-weight and stature depends on measurements taken on men in full health and at the prime of life, it was manifestly impossible to obtain the brain-weights of the same series. Through the kindness of his friend Herr Otto Ammon, he was able to measure four groups of young healthy men (inhabitants of Baden) who were classed as very tall, tall, medium and short. The group of very short persons was lacking. Below is given a part of his table for these four groups, each group containing 10 persons.

Group.	Stature. Cm.	Sitting Height. Cm.	Body Weight. Kgms.
1	177.4	91.7	72.74
2	171.9	89.9	67.49
3	165.7	87.7	60.11
4	159.5	85.05	55.50

In addition to the measurements exhibited above, the length and breadth of the head were taken and by the aid of formulæ and data derived from the results of Welcker and others, a calculation of the probable brain-weights for this series of men was made. This gave a series of four average brain-weights corresponding to the four groups, any one of which weights could be compared with all the others. Six comparisons could thus be made and the value of r the 'exponent of relation' determined in each instance.

In this series it was found to range between 0.1607 and 0.3978, with an average of 0.2586. There is evidence that the limiting values above given are excessive, and that with more numerous data the values approximate 0.25. Dubois, therefore, takes 0.25 for the value of r. If, then, we compare two groups of men (in this instance, males of the same race and locality) with one another, with a view to determining their relative brain-weights we find the relation can be expressed by the formula:

$$E : e :: S^{0.25} : s^{0.25} \text{ or } \sqrt[4]{S} : \sqrt[4]{s}$$

where the brain-weight of the heavier group is represented by E and

that of the lighter group by e, while S and s represent the body-weights of the heavier and lighter groups respectively.

The formula indicates that the increase in the brain-weights is in the same ratio as the fourth roots of the body-weights. This result is most interesting in itself, for by means of it we are able to replace the older and more general statement that among men the taller and heavier persons have on the average heavier brains, by the above formula, which expresses the law according to which the brain-weight increases. It is important to note that satisfactory data for testing this relation are extremely difficult to obtain, even under the most favorable circumstances, and for this reason it will probably be some time before it can be decided whether the same exponent of relation holds among women, or when the two sexes of the same race or different races from widely different localities are compared.

Yet, despite the limitations which are at once recognized, this determination by Dubois is an important discovery. One naturally asks why among men the increase in the weight of the brain in correlation with the body-weight, is so much slower than in the other mammals of different sizes which have been studied in connection with this problem. It can be said at once that in all probability the increase in the volume of the cranial cavity must take place according to this same law, and that, so far as the modifications of the nervous system are concerned, the larger individuals would appear to stretch the existing nerve-supply as the body became greater, rather than to add a proportional number of new elements for each new unit of area.

In this same paper Dubois examined the relation between the brain-weight and stature. A test of the data just employed shows that the groups increase in stature more rapidly than does the brain-weight. If, however, instead of the entire stature the sitting height be taken, Dubois finds that on the average the brain increases in weight in the same proportion as does the sitting height, a most simple relation, and one which Dubois regards as very important. If we may comment on this result, it would point in our opinion still more strongly to the explanation given earlier for the slow increase in brain-weight in taller and heavier men, namely, to an increase in weight due to mere passive enlargement of the central system as a result of the enlargement of the bony cavities containing it.

Working along this same general line, Donaldson has been able to obtain a simple formula which expresses for the frog the weight of the entire central nervous system as dependent on the body-weight and length of specimens of different sizes. This work differs

from the foregoing in that it applies to a form below the mammals, deals with the entire central nervous system—brain and cord combined—and expresses the change in the weight of this system in the growing animal.

It should be remembered, however, that the growth of the frog is different from that of the mammal, since at about 5 grams weight the frog presents those proportions of limbs and trunk which are maintained through the rest of its life. The difficulty in carrying out such an investigation lies in the determination of the normal body-weight. Not only are there here present all those difficulties which occur in the case of a mammal, but also variations in body-weight due to the amount of water, a factor which in the frog can undergo wide changes, and in addition, a distinctly marked seasonal variation in the relations between the weight of the central nervous system and that of the remainder of the body, so that for any study of this sort only mid-summer frogs are to be used, or observations on specimens taken at other seasons are to be reduced to the midsummer standard. When due care was taken to obtain correct and comparable body-weights, it was found, in the first place, that the increase in the weight of the central nervous system was nearly similar to the increase in the logarithms of the body-weights of the frogs compared; the series of logarithms did not, however, increase quite so rapidly as did the weight of the central nervous system. Another factor was therefore needed to make the two curves fit. This factor was found in the length of the frog, not applied directly, but with the value of the fourth root. The series of numbers obtained by multiplying in each instance the logarithm of the body-weight by the fourth root of the length, was found to be a nearly constant fraction of the observed weight of the central nervous system.

In the case of the bull frog the denominator of the fraction was 30, and hence multiplying in any instance by this constant would raise the number to the value of the observed weight. The formula for the bull frog is as follows:

$$C.N.S. = (\text{Log. } W \times \sqrt[4]{L})\, C$$

where the *C.N.S.* is the weight of the central nervous system in milligrams; *W* the weight of the frog in grams, and *L* its length in millimeters, and *C* a constant which for the bull frog has the value of 30. A similar formula applies to the leopard frog, save that in this case the constant is 28 instead of 30. In neither species is any difference according to sex to be observed. This indicates that during their

life the increase in the weight of the central nervous system is progressing in a regular manner, and that, too, despite the fact that the changes leading to an increase in weight are probably somewhat different in the large and small frog.

It suggests itself that the factor depending on length may be one which represents the mere passive increase in the system, whereby it adapts itself to the larger cavities in the bigger animals and thus increases in weight without increasing in complexity. If this be true, then we see that those changes depending on increase in complexity steadily diminish as the animal becomes larger. A further analysis of this group of changes must, however, await a better knowledge of the histological modifications which are there taking place. The outcome of the investigation is that any time after it has attained a weight of 5 grams the weight of the central nervous system of either *R. catesbiana* or *R. virescens* can be determined by means of a simple formula, thus showing a regular and orderly progression in those changes which lead to an increase in the weight of this system.

As regards the early growth changes in nerve elements, Hatai has been able to show that in the cerebellar cortex of the fœtal cat, the largest germinal cells present what Flemming calls a heterotypical mitosis. In these cells the number of chromosomes represented by the internodes of the segmental filaments is 16, this being the first determination of this number in the cells of the mammalian central nervous system. These general characteristics are probably the same in the other dividing cells in the central system.

In this connection some interest attaches to those structures in the cytoplasm forming centrosome and attraction sphere. It had been thought by some that since these organs of the cell play so prominent a rôle in cell division, they would probably entirely disappear from the cell when the ability to divide was lost. On searching for the centrosome in the white rat, both young and adult, Hatai found these structures in all classes of cells in the young rat at birth and also in all classes of nerve cells in the central system of the adult except in the cells of the corpus dentatum and the efferent cells in the ventral columns of the spinal cord, where the Nissl granules were so abundant as probably to obscure them.

In general the centrosome in the young rat is more easily distinguished than in the adult but maintains the same size, both in the different classes of cells and at different ages. In most cases the centrosome is composed of two corpuscles, and in the adult rat the attraction sphere and centrosome show changes which are interpreted

as signs of degeneration. It is hardly necessary to add that the cells in the adult which still exhibit the centrosome and sphere, have lost all power of further division.

A good deal of interest attaches to a determination of the time at which this power to divide disappears. On this point some observations of Miss Hamilton have a direct bearing. In her investigations of the nervous system of the white rat at birth and during the first four days after birth, she in the first place finds that cell division both in the brain and spinal cord was in active progress, even in the rat four days old. At first glance this would appear to be opposed to the current statement that in man cell division in the central nervous system comes to an end shortly after the third month of fœtal life. It must be remembered, however, that birth among mammals is an event quite independent of the maturity of the fœtus, and does not serve as a measure of development. The rat is born in a very immature condition, and before any proper comparison can be made between the two animals as to the time of the cessation of cell division in the nervous system, a careful determination must be made of the ages at which they pass through corresponding stages in their development, for in all probability it is the stage of development which is important.

The dividing cells in the older rats are found more and more frequently in the extra-ventricular portions of the section. Miss Hamilton's attention was largely directed to the determination of two sorts of dividing cells, the smaller giving rise to supporting elements, non-nervous in character, while the larger ones developed into neurones. In a number of instances she was able to observe mitotic changes in large cells having several branches, cells which would be classed as multipolar nerve cells. If the changes in the nucleus may be taken to indicate that such a multipolar cell is about to divide, then division in clearly differentiated nerve cells can occur, at least in the central system of the white rat. Opposed to such a conclusion is the accepted teaching that in the developing neurone the nerve fiber process or axone is the first outgrowth to appear. For if this were true it would be hard to understand what would happen to an axone under these circumstances.

It was difficult to attempt any reconciliation between the current teaching of the early development of the axone and those differentiated cells showing signs of division, until evidence was brought forward for the view that in certain parts of the central nervous system, the cerebral cortex for example, it was not the axone but the dendrites which were the first to appear. Bechterew in 1899 was led to

this view from the study of silver preparations of the developing human cortex, and Paton in 1900 reached the same conclusion as the result of his investigation of the histogenesis of the cellular elements in the cortex of the pig. Hatai has been able to corroborate and extend these observations and to show very clearly in the cerebral cortex of the fœtal cat that it is not until after the dendrites are formed and until the neurones are aggregated at a distance from the ventricle in the fourth and fifth cell-layers of the cerebral cortex that the axone appears.

It will be noted that these observations on the late development of the axone apply to the cerebral cortex only, and the question has not been retested since the classic observations of His on developing cells in the spinal cord gave rise to the current view.

Nevertheless, since it is possible that in other parts of the central system the same condition may occur, the presence of differentiated nerve cells, *i. e.*, those with dendrites undergoing division, can be explained without doing violence to any of the established views, for their dendrites may develop before the axone is formed.

As is generally recognized, the growth changes in the central nervous system are the result of two different processes not strictly separated but having a very different value at different periods. In the first period, enlargement of the system is the result of the multiplication of the cell elements by cell division, while in the second period the enlargement is at first mainly, and later, entirely, due to the development of cell elements already formed. Thus in a general way, out of the total number of elements formed by the early cell division, only a fraction undergo immediate development—the completion of which, in the larger mammals, may require years—while for a time, at least, new elements continue to enter upon the changes leading to complete development. Thus while the total number of neurones, immature and mature, is constant, the number of mature neurones steadily increases at the expense of the immature group.

This is the general method according to which the human nervous system is thought to be transformed from its condition at birth to that at maturity. When this series of events is analyzed, and especially when the changes occurring during old age are included, a great number of problems arise, each of which must be settled by a detailed investigation.

In determining the number and size of the spinal ganglion cells and dorsal root fibers in the white rat, at different ages, Hatai has dealt with one of the problems in question. The investigation takes de-

parture from two fundamental points. First, that at and after the age (about 8–10 days) of the youngest rat examined, there is no internal evidence that any cell division takes place in the cells of the spinal ganglion, and second, that all the medullated fibers in the dorsal nerve roots of the spinal cord are outgrowths of cell bodies located in the spinal ganglia. The evidence for fibers in the dorsal nerve roots having a different origin is, in mammals, at present too slight to impair the validity of the second point. If these points are accepted, then from the age when the rat weighs ten grams on to maturity, the number of cell bodies should remain constant. This can be determined only by comparing corresponding spinal ganglia in different rats of different ages. In this case we should hardly expect the number of cells to be exactly constant, since individual variations must occur. A study of the number of ganglion cells in four rats, the cells in three corresponding ganglia being counted in the case of each rat, shows that the number of cells in a given spinal ganglion does not increase with age, but remains constant. If the general view previously stated be correct, namely, that the neurones of this group mature in series, so that as the animal becomes older a larger and larger number of mature elements is to be found, then we should for one thing expect the number of medullated fibers in the dorsal roots to increase. Such is found to be the case. Taking all the fibers in the dorsal nerve roots, without distinguishing those which are entirely mature from those which are still immature, Hatai finds that in a rat of 167 grams body-weight there is something more than twice the number which appears in the nerves of a rat of 10 grams body-weight. If, however, attention is directed to the fibers which are entirely mature, the rat of 167 grams has from 4 to 5 times (according to the nerve) the number found in the young rat of 10 grams. Here, for the first time in the case of a mammal, Hatai has been able to follow the slow constructional changes by which the afferent system is put into connection with the spinal cord. This is seen to be accomplished by the prolonged post-natal development of the neurones, all of which are represented in the ganglia of the youngest specimen in the series. A host of questions at once arises. Do we know that we have reached the limit of this addition of new fibers? The question can be answered only by the study of older and larger rats than that which forms the upper limit of this series. Are all the cells of the ganglia represented in the dorsal nerve root by medullated fibers? In reply to this question, Hatai can show that since in a given ganglion the number of cells is constant, while the number of fibers in the dorsal root in-

creases, the ratio of fibers to cells must be a diminishing one, but in no case does he find less than 2.7 cells for each fiber that appears in the root. This agrees with results of other investigators who have always found more cells in the ganglion than there were fibers in the dorsal root. How far this group of neurones, which does not contribute to the dorsal root fibers, does contribute to the internal complexity of the ganglion, and how far the extra cells are both physiologically and morphologically immature, further investigation must decide.

No one can look at the growth changes here taking place without feeling that variations in the amount of exercise would modify the results, and also wondering to what extent these changes in the growing nervous system of the rat are exhibited by the growing nervous system of man.

It seems most probable that such changes occur in man, but further investigation alone can show at what period the afferent pathways represented by the spinal nerve roots become numerically complete, or the rate at which that completeness is attained. It is by studies in this field, and, so far as possible, on human material that the information can be obtained to show how far the nervous system is growing during the period of formal training, and therefore what possibilities there may be of modifying its later growth by exercise and stimulation.

From quite another side Cunningham, in his presidential address given before the Anthropological section of the British Association for the Advancement of Science at the meeting of 1901, discusses some growth changes which affect the human encephalon as a whole. He there points out that in considering the encephalon and its relations to the cranium we must regard the encephalon as the important factor, and that by its growth the form of the bony envelope has been largely determined, thus the lofty cranial vault of the human skull is a consequence of the great development of the cerebral hemispheres in man. If the cranium in its various modifications can be looked on as the outward expression of the contained encephalon, a newer craniology may, with this idea as a guide, describe more accurately than heretofore the variations in the development of the encephalon in the different races of mankind.

In this connection Cunningham takes up certain growth changes in the cerebral hemispheres which he believes to be especially characteristic of man, and which 'unquestionably have had some influence in determining head forms.'

The primate cerebrum, in contrast to that of the lower mammals,

is distinguished by the possession of a distinct occipital lobe and by a convolutionary design which in all but a few fundamental features is different from that of any other order of mammals. Up to the time when the occipital lobe is formed on the cerebral hemispheres—that is, the time when the cerebrum changes from the quadrupedal to the primate type, the cranial envelope is closely applied to the surface of the cerebrum. At this period it would appear that there existed an antagonism between the cerebrum and the enclosing walls, although Cunningham still considers the evidence which has led to this view as insufficient. With the development of the occipital lobe, however, these relations undergo a complete change. The cranium expands more rapidly than the cerebral mass, a considerable space filled with spongy subarachnoid tissue is formed between the two and into this mass the first convolutions of the cerebral surface are pushed out. This opportunity for free growth, however, becomes less and less as the end of the pre-natal period is approached, so that at the time of birth the gyri which have been formed are closely compacted and the cranium is so moulded over the surface of these gyri that the outline of the summits of the gyri is impressed on the inner surface of the cranial wall. Pursuing further the peculiarities of the human cerebral hemispheres, Cunningham points out that the other primates also exhibit a well-developed occipital lobe. Indeed, it appears that in man the region of the occipital has suffered a comparative restriction through the encroachment of the parietal region upon it. It is to this latter region that Cunningham especially calls attention.

The recent work of Flechsig on his association-centers, the whole literature of aphasia, as well as the studies of Rüdinger on the brains of eminent persons, serve to indicate the great importance of this portion of the cerebral surface. Nevertheless, Cunningham does not find any warrant for Rüdinger's sweeping statement that the higher the mental endowment the greater is the relative extent of the superior portion of the parietal lobe. On the contrary, he finds that it is the lower part of the lobe which in man, both in the earlier development and the after-growth, shows the greater relative increase.

He doubts whether the evidence is sufficient to support the views of certain authorities that an ample development of this region is to be found in the brains of men of unusual ability, but at the same time the peculiarly generous development of it in the human cerebrum makes it a region worthy of careful study. In the further examination of the inferior parietal lobe, it is pointed out how in connection with the frontal lobe it forms the superior operculum of the insula, and how in

this way it grows downwards to meet the inferior operculum formed by the temporal lobe and so to give rise to the Sylvian fissure. The interesting point here is the angle formed by the Sylvian fissure with a horizontal plane; this angle in the young human brain, as in the primate brain generally, is large, but in the human brain alone becomes less and less as the brain matures and thus the Sylvian fissure approaches the horizontal plane. This change in direction towards the horizontal is referred to the more rapid growth of the inferior parietal lobe in its posterior or occipital portion, this part of the operculum being more energetic in its growth than that formed by the temporal lobe. As a result, Cunningham states as his own observation the very interesting fact that in the left cerebral hemisphere the Sylvian fissure, as marked by the lower boundary of the parietal lobe, is more depressed than in the right hemisphere, thus showing an overgrowth on the left side. This overgrowth, he argues, is associated with the functional differentiation of the left hemisphere, the greater physiological importance of which, especially in connection with the faculty of speech, he considers very important. This leads to a discussion of language, and the peculiar importance of the development of it in man, with which topic the address closes.

Returning now to the statements concerning the better development of the inferior parietal lobe in the left hemisphere of man, we may be permitted to raise the general question of differences between the right and left hemisphere. As Cunningham himself states, it can be shown that the superiority of the left hemisphere depends neither on greater weight, greater convolutional complexity, better blood supply or better development (*i. e.*, higher specific gravity) of the cerebral cortex.

This being true, then the greater growth of the inferior parietal lobe must be compensated for by a smaller development of some other part of the hemisphere. Moreover, according to Eberstaller (1890) the principal external portion of the Sylvian fissure is longer on the left side than on the right, and longer in the brains of women than of men. The greater length of the fissure would certainly suggest a less complete development of the opercular masses, and in so far point to a smaller rather than a greater cortical extension on the left side.

Moreover, while all that Cunningham says concerning the great significance of the cerebral mechanism for speech and for the control of the finer forms of expression is most acceptable, yet the neurones immediately concerned in the control of the muscles from the cortex are predominantly in the frontal lobe and not in the parietal, so that

the enlargement of the parietal region is at best an associated change, and since the weight of the left hemisphere does not surpass that of the right, it is also a change which must be compensated by the smaller growth of some other point. HENRY H. DONALDSON.

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PSYCHOPATHOLOGY.

Psychopathological Researches: Studies in Mental Dissociation. BORIS SIDIS. New York, G. E. Stechert. 1902. Large 8vo. Pp. 329 and ten plates.

In the preface to this handsomely printed volume we are informed that the publication has been made possible by the Trustees of the New York Infirmary for Women and Children (and especially by Dr. Alexander Lambert). The Psychopathic Hospital and Laboratory established by them continues the work of the former Pathological Institute of the New York State Hospitals. Dr. Sidis is the director of the Psychopathic Laboratory and in presenting this first volume of studies, announces another work to be entitled 'Principles of Psychology and Psychopathology.' While this future volume will naturally be of more direct interest to psychologists, the present series of case-records presents many starting points for suggestive psychological thought. The central doctrine to which the several cases contribute is that of dissociation; and as a result of these experimental inquiries that conception acquires at once a more definite, a more comprehensive, and a more important significance. Unquestionably the psychology of the subconscious forms one of the notable problems of contemporary inquiry; and its illumination from the side of the abnormal has, up to the present, been the most distinctive aspect of the inquiry. Along with an appreciable volume of critical investigation and judicious generalization has been put forth a far greater mass of half-baked theories and conclusions, in which all sorts of discoveries of coexisting personalities, subliminal selfs, splintered egos, strata of the unconscious, run riot; while hypnotism is yielded as a potent weapon that opens out secrets of mind as easily as a blow with a hammer lets the milk flow out of a cocoanut. It is fortunate that Dr. Sidis has determined to take up this problem from the point of view of the psychological alienist; to apply to it the methods of inquiry suggested by psychological analysis, and to interpret the phenomena presented as extreme or aberrant forms of mental interrelations the analogues of which are to be found in normal mental functioning.

Dr. Sidis's remarks upon the suggestiveness of the simpler forms of phenomena are particularly apt in the field of psychology. It appears, moreover, that this method of research reveals modes of successful treatment, where the ordinary methods have failed.

The cases described belong to the vast area of functional psychoses; they include cases of hysteria, of amnesia resulting from an overdose of alcohol, of delusional states with variable emotional concomitants, of psychic epilepsy, or simulants of epilepsy, of motor disturbances, pseudo-paralysis, etc. In all an existing, more or less transitory (and sometimes recurrent) state of consciousness differs from and is seemingly unrelated to the normal, usual consciousness; the bridge between the two is in typical cases invisible, the transition abrupt, the habit of thought and conduct seriously altered. What Dr. Sidis and his associates have attempted in the cases described, is to determine how far these abnormalities find their origin in subconscious experiences; when such are established, the subconscious storehouse is 'tapped' by aid of hypnosis, or of ingenious variations of hypnoidal conditions. Thereupon, first, by means of proper suggestion the genesis of the psychopathological state, which the patient is unable to report upon in his present condition, is gradually brought to light; secondly, these dissociated experiences are reconnected with the normal consciousness (synthesis of the dissociated states); thirdly, the abnormal factors in the composite are by the same means suggested away, and recovery takes place. This bare description seems both vague and theoretical; it needs to be applied to the special cases to ensure a realization of its significance. For this the reader must be referred to the original, as the cases can hardly be reproduced in outline; the thread of detail and the sequence of stages are needed to produce an intelligible and convincing narative.

The importance of this volume will depend upon the possible extension and corroboration of the methods and results therein presented. If, indeed, it can be established that in a large proportion of mental difficulties (the true psychopathies), the source of the aberration lies in the fact that the subconscious background slips away from the normal relation to the foreground, until by its emotional persistence or otherwise it usurps the place of the foreground; and if, further, restoration to normal conditions be possible through an appeal to the deranged mental products by hypnosis, then the value of the analytic psychological method will receive a most notable confirmation, and the term 'psychotherapeutics' acquire a more rational meaning and a valuable extension. How far such will prove to be the case the

future will decide; and Dr. Sidis's attitude toward his data will probably be set forth in the promised volume.

The psychological reader of this volume will find some presentations that do not command his full acquiescence; he will find details that seem to him superfluous or meaningless; he may have doubts as to the precise nature of the 'hypnoidal' states described and their possible production in normal individuals. He cannot fail, however, amid such diversity of opinion as he may discover, to find an unusual collection of psychological material, and of suggestive illustrations of established and tentative psychological principles. A volume having these characteristics is worthy of careful scrutiny.

JOSEPH JASTROW.

UNIVERSITY OF WISCONSIN.

La perception visuelle de l'espace. B. BOURDON. Bibliothèque de Pédagogie et de Psychologie. Paris, Schleicher frères. Pp. 460.

The first seventy pages of the book are devoted to a description of the structure and functions of the eye. Here, as throughout the book, the author has brought together in condensed form, material from all the more elaborate treatises and from the scattered periodical literature bearing on his subject. To a review of the borrowed material he has added many carefully formulated criticisms and many original experimental data.

The most important original contribution in the first section of the book is the conclusion that the eyelids are sources of tactual sensations which play an important part in the visual perception of position. The lids are very sensitive on their inner surfaces according to the observations reported, and they are very active during movements of the eyes. In this connection it may be said that of all the tactual sensations involved in visual perception, those from the eyelids seem to be recognized by the author as the most important. Muscle sensations are everywhere treated as relatively unimportant, contributing little except, perhaps, in the case of recognition of depth, where convergence and accommodation are both recognized as contributing, together with other tactual factors, muscle sensations of some importance. It is retinal stimulation which the author repeatedly emphasizes as the main source of the sensations involved in visual recognition of space. When tactual sensations are recognized at all, they are chiefly the sensations from the lids.

After the first general description of the eye and its functions there

are chapters dealing with plane figures, with perception of movements and of depth, and with illusions. The separation of plane figures and geometrical illusions is a very serious defect in the book. It is evidently due to the author's great interest in such problems as the extent of the minimum visibile, the accuracy of the recognition of vertical and horizontal directions, and the degree of ability to arrange points in different parts of the field of vision in straight lines. These problems are the ones on which the author reports original observations. The observations reported are, for the most part, the author's personal observations. In a few cases one or two other subjects took part in the investigations, but in no case are the results sufficiently numerous or unique in character to modify the commonly accepted conclusions on all these matters. The author's lack of special interest in geometrical illusions, on the other hand, appears in the fact that he does not attempt any original contributions to this part of his work. He is content to be a mere reviewer in this field. The result is that the theory of the visual perception of plane figures is not greatly advanced by the book. So far as the general position of the author is definitely formulated, it may be said to favor the doctrine that space perception depends mainly on retinal sensations and only very secondarily and indirectly on tactual sensations.

In the treatment of the perception of movements and the perception of depth, the author again appears as an original contributor to the data. Convergence is regarded as a factor in both monocular and binocular perception of depth. With reference to the much debated subject of the relation of accommodation to monocular perception of depth, the author sides with Wundt. Though the importance of movement is thus recognized, greater emphasis is laid on retinal factors. The sensations from corresponding points in the two eyes are found in a series of experimental observations to be clearly different from each other, and sensations from disparate points do not fuse when the eyes are carefully fixated.

In the perception of movements of objects, tactual factors again receive some recognition, though the part played by those tactual factors is clearly regarded as subordinate to the part played by retinal factors. The relative significance of the two kinds of sensations is typically illustrated in those cases of illusion in which the real movement is in the eye and not in the object, but is, through a failure to recognize the eye movement, interpreted as belonging to the object. The dark-room experiment of trying to fixate a luminous point was repeated by the author with the usual result of involuntary eye move-

ments and interpretation of the movements as belonging to the objects. The directions of such movements were carefully recorded and traced to the involuntary and unperceived action of the eye.

The last part of the book reports facts in regard to the development of visual perceptions in children and in congenitally blind persons who have had their vision given them through operation; also the facts in regard to the recognition of the sizes of the sun and moon, and the facts of association between hand and arm movements and visual ideas. All the discussions here presented are summaries of the work of other investigators, except in the case of the discussion of the apparent flatness of the sky which is treated in the light of personal observations as well as in the light of what others have reported on the subject.

As a general text-book this number of the Bibliothèque has succeeded admirably in carrying out the purpose of the series to which it belongs. Much material has been carefully and critically worked over. It is evident that French and German sources have been more freely used than American or English sources. This will make it none the less useful to American students who may wish to complete their references on the subjects discussed. We are well enough supplied with English references in our own English text-books; the French writers particularly will be easily available through this work. The author's own experiments also present, as indicated above, valuable contributions to a number of the topics.

CHARLES H. JUDD.

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APPARATUS.

The Psychergograph: A method of measuring mental work. C. E. SEASHORE. Studies in Psychology, Iowa University, Vol. III., pp. 1–18.

A Voice Tonoscope. Ibid. Pp. 18–29.

These two articles may be reviewed together and in their order:

The 'psychergograph' is an instrument containing a circular disk 38 cm. in diameter, the circumference of which is divided into one hundred equal spaces, and upon each of these spaces is pasted one of four colors. These colors are arbitrarily arranged into a series, and as the disk is rotated one color after another is exposed through a signal slot in a screen covering the disk. Immediately behind this signal window are four keys containing each one of the four colors in the series on the disk. Every time one of these keys is pressed down the disk passes forward one one-hundredth of a revolution, thus exposing

the color on the next space of the disk. The idea is to press down the keys as their respective colors appear in the signal window. These keys are electrically connected, both with the disk, which passes one notch every time a contact is made on any key, thus bringing a different space of the disk under the window, and with four markers, each key with its respective marker. These markers make continuous tracings on a tape passing under them at a uniform rate, and a time record is also made on the tape. All the contacts of each key — errors, time relations, and fumblings — are recorded and can be determined at leisure.

This seems a good and handy instrument for the psychological laboratory and some valuable researches may be carried on by means of it. It can be used to make practice experiments, experiments on continuous reaction time, on fatigue and its effect, on rate and accuracy of discrimination, etc.

The advantage of the 'psychergograph' over the rate of reading, for example, is that it gives a record of the variability, but this brings no particular intelligence, unless treated *en grosse* to get results and standard. Dr. Gilbert ('On School Children,' *Yale Studies*, Vols. I. and II.) found only slight correlation between mental tests and reaction time. Association time goes better. Idiots have twice as long association reaction time as school children ('Feeble Minded People,' *Ped. Sem.*, Vol. III., p. 246, G. E. Johnson). Dr. Wissler found that reaction time is a very poor measure of mental efficiency since it does not correlate at all well with any of the other mental tests of Columbia freshmen ('Correlation of Physical and Mental Tests,' PSYCHOL. REV., Mon. Sup., Vol. III.). Rate of reading seems one of the most satisfactory tests so far given, and it is not unlikely that tests that might be given on the 'psychergograph' would be as unsatisfactory as any in finding a norm of mental work. Tests of mental work ought, as far as possible, to eliminate muscular work.

The 'voice tonoscope' referred to above is made up of a stroboscopic screen of heavy white paper tightly fitted around a metal drum, mounted on ball-bearings, 50 cm. in radius and 50 cm. wide. On this screen and equidistant are placed 71 rows of dots 3 mm. in diameter, so that when the drum is revolved all these rows will form parallel lines 3 mm. wide. These rows of dots are divided into two series, the first of which contains 36 rows and the second 35. The first row of Series One has 73 dots, equidistant, in its circumference, and each succeeding row has one more dot. The rows of Series Two alternate with Series One, beginning with a number of dots where the

first series ended and each successive row increased by one dot, thus ranging the number of dots in the rows of the two series respectively from 73 to 145 in a revolution of the drum, and forming dot frequencies when the drum is revolved at one revolution per second, which correspond to the vibration frequencies of the human voice. To show the dot frequency of any row in a revolution a scale showing the number in that row is placed horizontally against each side of the drum.

To revolve the drum at the rate of one revolution per second a vacuum tube is placed above the scale on one side of the drum. This tube is electrically connected with a 100 v. d. fork. Then the drum is started and speed increased until the dots in the row having 100 dots in its circumference seem to stand still. This becomes the standard rate of speed. On the opposite scale is placed a manometric capsule, connected with gas and speaking tubes. The observer holds the speaking tube to his mouth and sings the tone that is to be determined. The gas flame rises and recedes once for every vibration of the vocal organs, and when the screen is moving at the standard rate the row of dots will seem to stand still on the screen whose dots correspond to the frequency of the vibrations of the tone and the pitch is indicated on the scale by the number over the line standing still. There is also a telephone connection between the standard, *i. e.*, 100 v. d. electric fork, and the observer can at any time get the standard tone through the receiver. The experiments are carried on in a darkened room.

This is an instrument very ingeniously devised and is bound to be as useful as the Hipp chronoscope. Experiments for which it can be used and which have been pointed out by its inventor are: (1) ability to sound a tone, (2) variability in sustained tones, (3) singing tones at certain pitch intervals, (4) ability to run scales and to sing melody, (5) speaking in a uniform pitch, (6) the least producible difference in pitch.

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CHILD PSYCHOLOGY.

La logique chez l'enfant et sa culture. Étude de psychologie appliquée. FRÉDÉRIC QUEYRAT, Professeur de philosophie au Collège de Mauriac. Paris, Félix Alcan (Bibliothèque de philosophie contemporaine). 1902. Pp. 158.

This book belongs to the author's series of monographs on applied psychology, and is closely connected with his 'L'imagination et ses variétés chez l'enfant,' and his 'L'abstraction et son rôle dans l'éduca-

tion intellectuelle.' While the first of these treated of the rise of images, and the second of the formation of general notions by association of ideas and other means, the present volume goes on to investigate the rise and development of the power of relating notions and judgments in what we call reasoning proper. The child's intellectual life is divided into three periods, according as *sense, spontaneous thinking* or *reflective thinking* plays the chief rôle. The second of these periods begins about the third year; the third from four to seven years later. The logic of the earliest childhood is that of loose analogies, personifications and anthropomorphisms, and is subject to numerous errors and sophisms. Out of this the young reasoner slowly emerges, as the principles of identity, causality, and finality assert themselves, dominate his thinking and correct the vagaries of his earlier analogical reasoning, which was controlled almost entirely by chance associations of images of sense. Chapter three gives a very interesting account of the main sources of logical fallacy, and might be accepted as a fairly good statement of this part of the subject, as applied not only to the child, but to the adult as well. This is followed by a description of the various logical types, with historical illustrations; and the closing chapter of the book is devoted to a discussion of the chief subjects of study, with respect to their relative value as means to the training of the reasoning powers. While one can hardly say that much new material is presented in this book, it is nevertheless well worth reading on account of the skillful manner in which familiar material is arranged and classified, and pedagogical deductions drawn therefrom.

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FEELING AND EMOTION.

Les émotions. G. SERGI. Paris, Octave Doin. 1901. Pp. 460.

This is a French translation of the author's 'Dolore e piacere' with an added chapter dealing with recent criticism of the theory — the James-Lange, or, as the author prefers to call it, 'peripheral' theory of emotion — which it is the principal aim of the work to expound and defend. As the Italian original was reviewed at length in these pages (Vol. II., p. 601), at the time of its appearance, it will suffice here to remind the reader, with the briefest possible comment, of the principal features in Sergi's holding of the doctrine, and to call attention to what is new in his reply to objections. It is to be noted, in the first place, that while James rested his theory mainly on the evidence of intro-

spection — we always experience organic perturbation whenever we are emotionally moved, and we can't imagine ourselves emotionally moved if we think all organic sensations absent — Sergi relies largely on more or less philosophical assumptions in the interpretation of biological and physiological facts. His view at bottom is that the whole mental life is a transformation of primitive organic irritability and that the latter in its turn is a transformation of physical energy. Irritability, he teaches, conjoined with consciousness becomes in the nervous system sensibility, with sensation, involving cerebral centers, as its function, consciousness being not a distinct phenomenon in itself, but a property, or manifestation, of a phenomenon taking place, or rather that has already taken place, in the nervous centers. Sensation and consciousness thus conceived, the primitive phenomena included in sensation are held to develop in two directions: (1) Perception and the intellectual phenomena, (2) sentiment, or feeling (primarily pleasure and pain), and the emotional phenomena.

With these most general assumptions, Sergi proceeds to construct his theory. The first step is to connect pain and pleasure with the vital functions. Pain is held to be a form of irritability due in all cases, even those of inaction, to excessive and, therefore, harmful stimulation, pleasure a form of irritability due, negatively, to the return to normal organic conditions, positively, to novel excitements, the more precise nature of which is not here further defined. The indications thus afforded by the external conditions of pleasure and pain of their relation to organic welfare, more obvious in the case of pain than in that of pleasure, which is biologically valuable chiefly as a means of avoiding pain, are definitely established, in Sergi's opinion, by their relation to the vital processes — the inner movements of heart and lungs — controlled by the nervous centers in the region of the fourth ventricle. This relation is regarded as primary. That the function of the brain in bringing these movements to consciousness is only secondary is inferred from the fact that they are unaffected by ablation of the hemispheres. Sergi makes at this point the important assumption, which he appears to consider the facts mentioned to prove, that this vital center in the medulla is, or contains, at the same time the center of pleasure and pain. It seems to be suggested, though the thought is too vaguely expressed for one to be certain, that the feelings of pleasure and pain are either the consciousness of the cardiac and respiratory movements, or of the sensations to which they give rise, of the changes in the centers in which they originate.

Emotion, now, being regarded as a further development of the

pleasure-pain feeling, the theory of emotion is given in the definition of the special conditions and other features characteristic of the more developed state. Three elements especially appear in Sergi's descriptions as these additional factors: (1) the perception or idea which serves as the stimulus, (2) the associated habits or dispositions — 'psychic organisms,' they are called — which give to the action its instinctive character, and (3) the diffuseness of the organic discharge, which includes in its effects secretory and other changes as well as affections of the heart and lungs. Central in the whole process, for the author, is the affection of the vital center in the medulla, the importance of which for the functions of life is emphasized by diagrams and by a special chapter of description. Emotions, then, are regarded as having the same origin, as well as the same basis, as pleasure and pain. They are primarily for defense. They also have the same exaltative or depressive character, a principle which our author uses for their classification. Painful emotions have their genesis in change of vital function consisting in a diminution of activity (though pain, we remember, is always due to excess of stimulus), agreeable emotions in real or apparent augmentation of activity. The consciousness of the emotional process is, of course, secondary, just as in the case of pleasure and pain, but just as in that case also, it is not clear at this point whether it is to be thought of as consciousness of the process in the medulla or of the organic movements or sensations as they occur in the other parts of the body.

Such in its main outlines is the theory. In the chapters following those which contain the above foundation principles Sergi seeks confirmation of his view in special analyses. Among other things he attempts, not unsuccessfully, to bring the æsthetic, moral and religious sentiments, which James had set apart as the 'subtler' emotions, under the general theory. And in the final chapter, the new chapter of the present edition, in approving many of the criticisms made against James, he claims superiority for his own view in its manner of conceiving the theory, in the determination of a common emotional center, in the use made of the hypothesis of psychic organisms and in the inclusion of the æsthetic, religious and moral emotions, treated as merely transformations of the emotions of common life and by no means always the subtler.

The advance beyond James's earlier statement is apparent. It consists essentially in the endeavor to relate the emotions to the principle of biological utility. The emotional life is so fundamental, so instinctive in its manifestations and so obviously connected with the

organic life of nutrition that its primary significance relative to conservation can scarcely be doubted. But while the author is to be commended for making this conception central, it is to be feared that he has left much to be desired in the working of it out. To begin with, if emotion is to be conceived as feeling with reference to an instinctive organic reaction relative to defence, the first requirement for a theory of the emotions would seem to be to define their place with regard to this self-preserving or, as the case may be, group-preserving attitude. There is little or nothing of this, in the sense here meant, in Sergi's work. There is, indeed, much said about the derivation of certain movements, especially the æsthetically non-useful, pleasure-giving movements of the dance, from movements having originally symbolically useful social significance, and there are here and there allusions to the abbreviations of originally useful movements in emotional expressions. But there is no attempt to synthetically relate stimulus and reaction, on the one hand, attitude, object and feeling, on the other, or to explain why, in view of their teleological character, emotions as we experience them are so disturbing. The author's tendency, on the contrary—it appears, in fact, to be a part of his express theory—is to assimilate emotions, so far as their teleological reference is concerned, to feelings of pleasure and pain, and to refer the rest to indiscriminate and diffused discharge of nervous energy. As to his doctrine of a special emotional center, identical with the center of pleasure and pain and the center of origin of the vital processes in the fourth ventricle, a doctrine on which he lays much stress, it is difficult to see what more such a center could effect, assuming its existence, than to serve as a 'funnel of discharge' for the cerebrally excited motor currents, which, according to Sergi, James, rejecting the idea of an emotional center, erroneously regards as reflex. There is clearly nothing in James's theory to preclude the idea of such a special 'funnel of discharge' in inferior centers. The question is purely physiological. But Sergi finds in the doctrine of a medullary center of emotion psychological significance. And he appears to think that the center in question is a true center of the life of feeling, a life which the hemispheres only bring to consciousness, notwithstanding the very obvious consideration that pain, pleasure and emotion are terms entirely devoid of meaning except as they denote just those states of consciousness themselves. An emotional center, therefore, in the proper sense, would have to be sought, on the author's own showing, in the hemispheres, where, it is needless to say, there is not the slightest evidence of its existence. And so, in the end, we find Sergi returning to James's

contention that the emotions are simply sensations of the viscera, *i. e.*, effects of the return wave of the excitement instinctively discharged through the organism from the brain. Thus, in reply to Stumpf, who criticised the general theory for its failure to supply a distinction between emotion and organic sensations not emotions, *e. g.*, a stomach-ache, he says, the distinction lies in the difference of the cause, also in the fact that in localized sensations producing pain consciousness is chiefly occupied with those localized sensations and not with the visceral sensations they give rise to, whereas in emotion it is more occupied with the visceral sensations, non-localized and diffused. The explanation suffers from the often pointed out defect in the statement of the theory, namely, the failure to connect the different elements in the process of emotions, since, apart from their relations in the emotional attitude, organic sensations, though visceral, are not emotional at all. But it at least serves to show the subordinate rôle of the so-called emotional center as a motor mechanism. This further appears in the reply to Ribot's demand that the psychophysical emotional process be conceived in a unitary way, without reference to the distinctions of cause and effect. Sergi contends that changes in circulation, respiration, etc., excite organic sensations, without being themselves organic sensations, just as light, etc., stimulate the external organs of sense. The organic sensations which constitute the feeling consciousness are thus at a second remove from the action of the vital center which excites the visceral changes and, of course, still further removed from the originally exciting perception or idea.

Besides James, Ribot and Stumpf, Sergi especially criticises, in the last chapter, Binet and Courtier, whose experiments on the relation of emotion to vaso-constriction, and Binet and Vaschide, whose experiments on its relation to blood-pressure showed, in the opinion of these writers, that no such relation obtained as the James theory required. He makes it clear that the facts are capable of being interpreted as either negative or as tending to confirm the theory. The rest of the chapter seeks to establish the theory by fresh evidence. First Mosso and Tanzi's observations, showing increase of temperature in the brain in connection with emotion, but no increase in temperature accompanying the increase in blood-supply and modifications of circulation in intellectual work, are cited as demonstrating the peripheral or sensualistic theory and giving to the intellectualistic theory its *coup de grâce*. They are also held to make for the author's view that psychic phenomena are a transformation of energy, like heat (p. 450). The facts are certainly curious, but hardly a warrant for such

a wide-reaching interpretation. Then the experiments of a number of investigators (Dogiel, Tarchanoff, Patrizi, etc.) are adduced to show the sensational organic character of the effects of music. Finally, the theory is confirmed by introspection, the author having noted, he tells us, on several occasions the presence of the idea in its completeness before the emergence of the emotion — an observation which one may modestly beg leave to doubt, if by the 'idea' be meant the consciousness of the object relatively to which the emotion is felt.

In spite of grave defects, among which is a certain specious simplicity masking vagueness of conception, as in the conception of a vital or pleasure-pain-emotional center, the conception of the relation of consciousness to sensibility, and the conception of psychic phenomena as a mode of energy, the book contains much that is valuable and suggestive. It contains probably the most complete exposition of the doctrine advocated in its various aspects, and the student interested in the subject is to be congratulated on now having it in a language more available to most than the original. H. N. GARDINER.

SMITH COLLEGE.

ÆSTHETICS.

Etudes esthétiques. GEORGES LECHALAS. Bibliothèque de philosophie contemporaine. Paris, Félix Alcan. 1902.

Under this title M. Lechalas has collected a series of pleasantly-written essays on topics of interest to both the critic and the psychologist. Beginning with two introductory chapters on the nature of beauty and art, he proceeds to discuss the relation of art to nature and to mathematics, the rôle of suggestion in art, the affinities of the various arts, art and curiosity, and art and morality. The treatment throughout suggests the critic who has read rather widely in modern experimental psychology and has used its results to illustrate and enforce principles adopted on grounds of personal taste, so that the essays, while interesting, are in no sense contributions to the science of æsthetics. They belong to the literature of criticism rather than to that of psychology, and draw their inspiration from Sully-Prudhomme and Fromentin rather than from Fechner or from Wundt.

The value of the author's theoretical basis may appear from the fact that having reduced beauty to the manifestation of being or perfection, and realizing the traditional difficulty as to the reality of the ugly or non-being, especially of the morally ugly, he takes refuge in a citation from Malebranche in which evil is given a *quasi* reality by casting it upon the free will of the individual. The essay on nature

and art is a summary of the difficulties which beset the artist in any attempt to reproduce the exact impressions from the object, as these difficulties have been brought to notice in Weber's law and the principle of relativity. Upon this practical impossibility of an exact realism is based a plea for a less mechanical interpretation of nature. In discussing the place of suggestion in art, the author finds it necessary to distinguish between the natural appeal to the attention which every work of art must make and that abnormal concentration of attention which we find in the hypnotic state. Reality must be suggested, but not so strongly as to destroy the distinction between it and the artistic representation—the beholder must not be hypnotized into belief in its reality. This principle of æsthetic *Schein* is of course good, as well as ancient, but when it is applied in criticism of the Bayreuth performances (which the author admits that he has never seen) it fails to convince. The concentration of attention upon the lighted stage in the dark and silent house is certainly far from destroying either the æsthetic illusion or the contagious influence of the audience.

The concluding papers on the relation of art to curiosity and to morality are purely non-psychological and also somewhat misleading in title. The former is a discussion of the place of subject and local color in art, the latter a rather loose treatment of some of the moral evils which are the possible results of art and its study.

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ETHICS.

Constitution de l'éthique: quatrième essai sur la morale considérée comme sociologie élémentaire. E. DE ROBERTY. Paris, Felix Alcan. 1900. Pp. 223.

M. de Roberty begins this the fourth essay in his series of ethical writings with a brief confession of his philosophic faith. His general position, often called modified positivism, sometimes attacked as a defection from positivism, he himself calls hyperpositivism—a name first applied to it in a depreciatory sense, but which he adopts as the symbol of his divergence from Comte. The positivist school errs in recognizing only one series of the products of intelligence, the scientific series; it ignores the 'grande' or 'psychosocial' series which includes philosophy, art, and industry, as well as science. In place of Comte's law of the three states M. de Roberty substitutes the 'law of correlation between the abstract sciences and philosophy.' Positivism inverts the

relation of these two terms of the mental series, the complete order of which should read: science, philosophy, art, industry. Philosophy and religion are determined by science, the æsthetic sentiments are determined by the prevailing religious and philosophical beliefs, and finally our practical activity reflects our sentiments and ideas: so that each term of the series depends upon all that precede it. Misconception of the mutual relation subsisting between the different members of the psychosocial series caused Comte to fail in the attempt to 'constitute' sociology as an abstract science. His first error was in separating morals from the totality of sociological studies, whereas, according to Roberty, ethics and sociology are in reality identical.

Sociology (or ethics) is not to be founded upon biology, nor should it be confounded with psychology (another of Comte's errors). It is a mistake to make either individual or collective psychology the basis of sociology; though it is less pernicious to make individual psychology the middle term, giving the primacy to collective psychology and considering the latter as the source of both individual psychology and of sociology, than it is to make collective psychology the middle term with individual psychology below and sociology above. Nor is sociology identical with *Völkerpsychologie;* the former underlies the latter. It is important to distinguish between the *fait social* and the *fait mental.* The latter is the necessary consequence, the inevitable product (manifesting itself in individual minds) of the contact, the reciprocal action, of living organisms already endowed with psychophysical faculties (*fait social*). Social phenomena precede, originate and sustain mental phenomena. But they are not identical with vital phenomena, nor are they to be found in some sphere vaguely said to lie between biology and psychology. The connecting link between biology and psychology is found in the phenomena of sociality, or altruism, or (if one prefers) of the moral sense. The 'biosocial' theory recognizes in sociality the true productive cause of the socially qualified individual, the person of rights and duties. The theory of 'collective psychism' (*psychisme collectif*) sees in sociality the *quid proprium* of sociology.

The three fundamental conceptions in the author's view of the nature of ethics are thus: (1) His view of the relation between the various factors of superorganic evolution, (2) his 'biosocial' theory, (3) the 'collective psychism' hypothesis. The combination of these leading ideas results in the doctrine that psychology is a concrete science studying the complex phenomena of mind—which are due to the concurrent action of the laws of life and of sociological laws; while soci-

ology is an abstract science of the vast world of superorganic facts, a science whose essential phenomena are identical with those of ethics. More briefly, sociality and morality are identical; so that, instead of blotting the word sociology out of the dictionary, as would be necessary if the collectivist psychology were right in identifying social phenomena with mental phenomena (p. 119), it would seem that the word ethics can now be blotted out.

Having dealt, in Chapters IV. and V., with the relation between morals and the other sciences contained in the first term of the psychosocial series, the author proceeds in the last two chapters to deal with the relation between morals and philosophy (including religion). Religion and metaphysics are here treated in the familiar patronizing manner of the positivists, who see in them once useful phases of thought now happily fast becoming obsolete. Until ethics has once and forever cut loose from metaphysics it will remain a normative science, encumbered by duties and imperatives, and saying 'One ought,' while the sciences arrived at maturity say simply, 'This is.' As to theology: Positivism failed because, among other reasons, it did not really do away with God. *Dépasser Dieu* is the real end of science arrived at the age of reason.

To indicate the general tenor of M. de Roberty's ethical views is not difficult; but to disentangle the constructive element from the superabundance of critical digressions and *obiter dicta*, and to read through the repetitions which are perhaps incident to the serial form in which the author has chosen to expound his ethical system, is a somewhat tedious task. His work would gain both clearness and strength from a rigid condensation and the more thorough, concise and connected discussion of certain leading ideas—such as, *e. g.*, the conception of sociality as equivalent to morality, and the identification of ethics and sociology.

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NEW BOOKS.

Outlines of Metaphysics. JOHN S. MACKENZIE. London, The Macmillan Company. 1902. Pp. xvi + 172.

W. Wundt's Philosophie und Psychologie. RÜDOLF EISLER. Leipzig, J. A. Barth. 1902. Pp. vi + 212.

Le dottrine edonistiche italiane del secolo XVIII. MICHELE LOSACCO. Naples, Tip. della R. Università. 1902. Pp. 125.

The Elements of Mind. H. J. BROOKS. London, Longmans, Green & Co. 1902. Pp. xviii + 312.

Les obsessions et les impulsions. A. PITRES et E. RÉGIS. *Le caractère.* P. MALAPERT. (Bibl. intern. de psychol. expér.) Paris, O. Doin. 1902. Pp. 434.

Einheiten und Relationen. TH. LIPPS. Leipzig, J. A. Barth. 1902. Pp. 106.

Personal Idealism. HENRY STURT. London, The Macmillan Co. 1902. Pp. x + 394.

Crimes et anomalies mentales constitutionelles. A. FOREL et A. MAHAIM. Geneva, H. Kündig; Paris, Alcan. 1902. Pp. 302.

Das Problem der Willensfreiheit in der neuesten deutschen Philosophie. LEO MÜFFELMANN. Leipzig, Barth. 1902. Pp. 116.

Martineau's Religionsphilosophie. ORLO J. PRICE. (Diss., Leipzig.) No date. Pp. 104.

NOTES.

THE seventieth birthday of Professor Wundt was celebrated in August by the presentation of a *Festschrift*, containing contributions by a large number of his former students. The presentation was made at Tambach in Thuringia by a delegation consisting of Drs. Külpe, Kraepelin, Ludwig Lange, Kirschmann, Meumann, Mosch, Wirth and Frank Angell. The contributions form two large volumes of the *Studien*, the nineteenth and the twentieth, and are to be the final volumes under Professor Wundt's direction. It is understood that the new series will be conducted by Professors Külpe and Meumann. The former students of Professor Wundt, resident in America, who contribute researches to the *Festschrift*, are Professors Frank Angell, J. McKeen Cattell, Ewald Flügel, Chas. H. Judd, A.

Kirschmann, E. W. Scripture, Edward A. Pace, G. M. Stratton and E. B. Titchener. Professor Wundt was made an honorary citizen of the city of Leipzig.

THE recently established British Academy for the promotion of historical, philosophical and philological studies includes in its membership Dr. Edward Caird, Mr. S. H. Hodgson and Professor James Ward. Mr. A. Balfour and Sir Frederick Pollock are also members, though presumably not on account of their contributions to philosophy. The names of Professor Alexander Bain and Mr. Herbert Spencer are missing from the list. It is, however, known that Mr. Herbert Spencer declined to become a fellow of the Royal Society.

PROFESSORS JOSIAH ROYCE and George H. Palmer, of the philosophical department of Harvard University, have leave of absence for the present year. Professor Palmer has sailed for England.

THE Rev. Edward A. Pace, Ph.D., has been appointed director of the Institute of Pedagogy which the Catholic University of Washington has established in New York City. The Rev. Thomas E. Shields, Ph.D., of St. Paul, has been appointed instructor of physiological psychology in the Catholic University of Washington, filling the place vacant by Professor Pace's removal to New York.

DR. FELIX ADLER has been elected professor of social and political ethics in Columbia University.

DR. FRANCIS L. PATTON, formerly president of Princeton University, has been elected president of the Princeton Theological Seminary.

DR. GEORGE S. PATTON has been elected professor of moral philosophy at Princeton University.

DR. J. W. L. JONES, Ph.D. (Princeton), has been appointed professor of philosophy and education in Heidelberg University, Ohio.

DR. FRANK S. WRINCH, Ph.D. (Leipzig), of Toronto, has been appointed demonstrator in experimental psychology in Princeton University.

PROFESSOR WM. URBAN, Ph.D. (Princeton), of Ursinus College, Pa., has been appointed to the chair in Philosophy at Trinity College, Hartford.

BEGINNING November 1, 1902, all editorial matter for this REVIEW, together with books for notice, should be sent to Professor J. Mark Baldwin, Princeton, N. J. Business communications should be addressed as heretofore to Professor H. C. Warren, Princeton, N. J.

INDEX OF NAMES.

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